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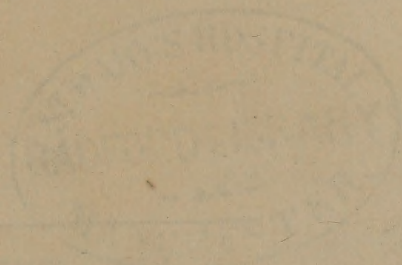
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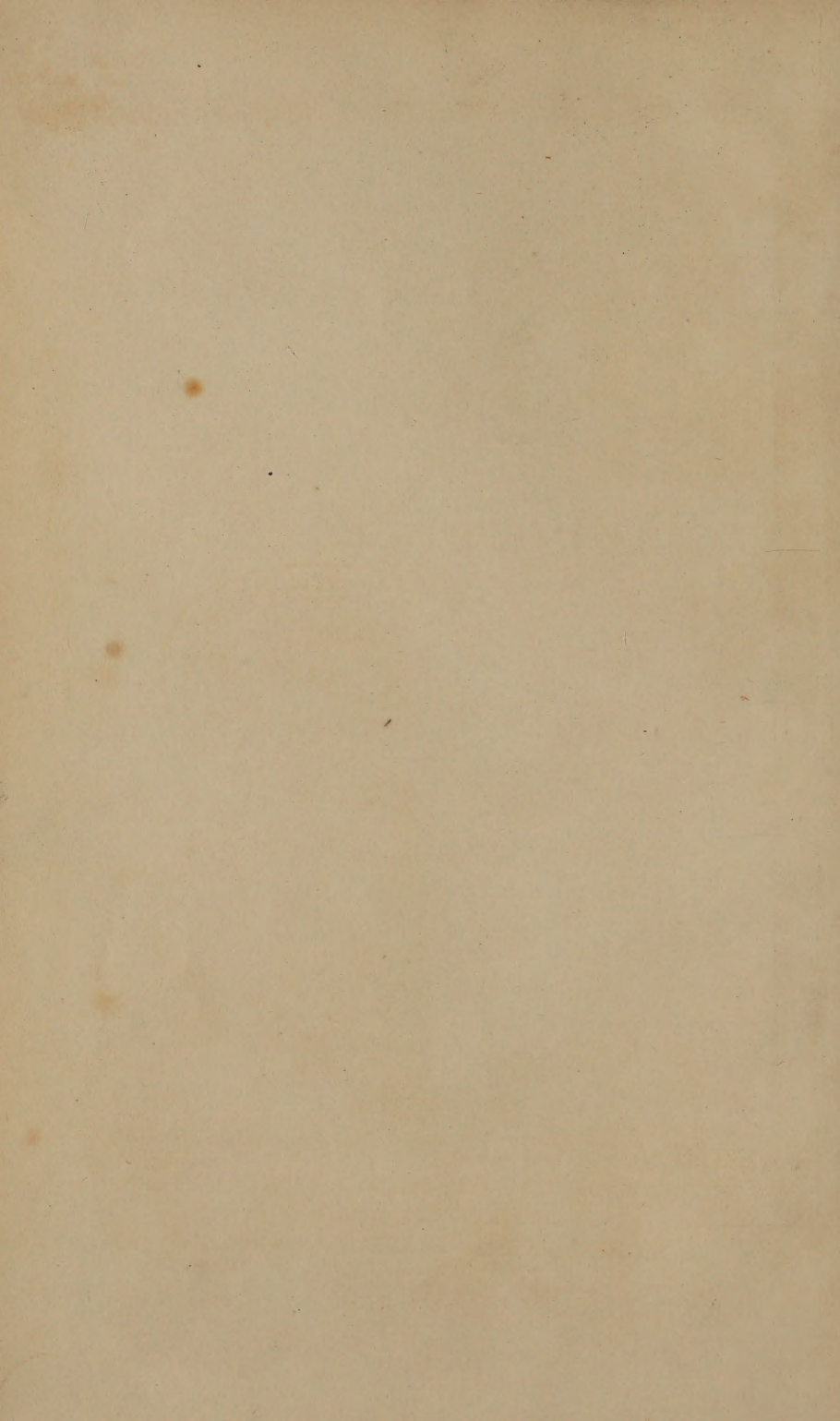
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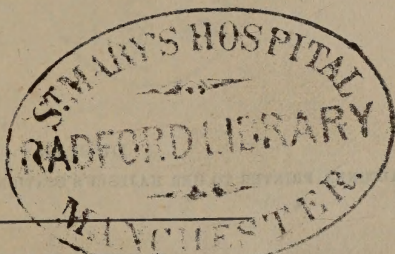
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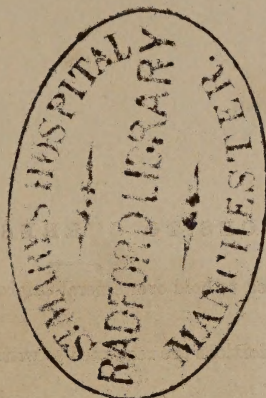
CONTENTS.

No. LXXII.—MAY 1, 1866.

PART I.—ORIGINAL COMMUNICATIONS.

327	Foot, M.D.; Fellow of the King and Queen's College of Physicians, Dublin.	Art. 17.—On the Pathology of the Liver.
314	W. B. Richardson, M.A., and M.D.; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases.	Art. 16.—Remarks on the Pathology of the Liver.
309	W. B. Richardson, M.A., and M.D.; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases.	Art. 15.—On the Pathology of the Liver.
300	W. B. Richardson, M.A., and M.D.; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases.	Art. 14.—On the Pathology of the Liver.
288	W. B. Richardson, M.A., and M.D.; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases.	Art. 13.—On the Pathology of the Liver.
284	W. B. Richardson, M.A., and M.D.; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases.	Art. 12.—On the Pathology of the Liver.
257	W. B. Richardson, M.A., and M.D.; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases.	Art. 11.—On the Pathology of the Liver.
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M^r. PORTER ON LITHOTRITY.

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THE
DUBLIN QUARTERLY JOURNAL
OF
MEDICAL SCIENCE.



FEBRUARY 1, 1866.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Observations on Crushing Stone in the Bladder.* By
GEORGE H. PORTER, M.D., T.C.D.; F.R.C.S.I.; Senior
Surgeon to the Meath Hospital and County of Dublin Infir-
mary; Surgeon to Simpson's Hospital; Examiner in Surgery
Royal College of Surgeons, Ireland; Consulting Surgeon to the
Coombe Lying-in Hospital; Member of Council of the Surgical
Society of Ireland; Member of the Pathological Society of Dublin;
Lecturer on Clinical Surgery.

CASE I.—PATIENT AGED SIXTY-EIGHT YEARS; LITHATE CALCULUS,
OF MEDIUM SIZE; LITHOTRITY; RECOVERY.

CASE II.—PATIENT AGED THIRTY-SIX YEARS; OXALATE OF LIME
CALCULUS, OF SMALL SIZE; LITHOTRITY; RECOVERY.

CASE III.—PATIENT AGED SEVENTY-FOUR YEARS; LITHATE CAL-
CULUS, OF SMALL SIZE; LITHOTRITY; RECOVERY.

TWENTY years ago the late Sir Philip Crampton delivered an able
lecture, in the theatre of the Meath Hospital, on lithotrity, which
he published, with accounts of twenty of his own cases, in the
February number of this Journal for 1846. Since that period I can
find but few records of the operation by surgeons in this country,
so that we are indebted chiefly to our English and continental
brethren for information concerning this operative procedure, and

the after-management of the patient.^a This consideration in some measure induces me to note and remark on the following cases which came under my own care during the last year, and which proceeded to a successful termination. A certain amount of interest, it may be presumed, attaches to the subject, inasmuch as eminent medical men of the present day are differing widely in opinion as to whether lithotrity has any advantage or superiority over the operation of cutting for the stone.

They hold, also, very opposite views respecting the proper mode of performing the crushing operation in its various stages. And yet, perhaps, this should not be wondered at when we bear in mind that lithotrity may be said to have been in its infancy not many years ago. But no operative procedure has been more improved upon of late, owing to the ingenuity of mechanical contrivances, and the practical hints of scientific surgeons. The clumsy appliances of forty years ago are now only shown in order to contrast them with the superior instruments of the present day; and it is satisfactory to know that some of the apparatus of the greatest utility to both patient and surgeon have been devised in this country. The name of Mr. L'Estrange at once occurs to me in this connexion, as one to whom the surgical profession will acknowledge they are deeply indebted for many valuable instrumental improvements. Mr. Oldham, formerly of the Bank of Ireland, has also in this way rendered important services. The aged sufferer can now have a stone in his bladder shattered by lighter and more manageable instruments, and is saved the danger of the formidable operation of lithotomy. In theory, nothing can be easier than breaking a calculus in the bladder, but the operating surgeon does not usually find it to be so when he comes to deal with such a case. Every minute detail, from the moment he measures the size of the stone, and endeavours to ascertain its composition, requires the utmost watchfulness, steadiness, and dexterity.

CASE I.—Patient aged Sixty-eight Years; Lithate Calculus, of Medium Size; Lithotrity; Recovery.—A gentleman, aged sixty-eight years, labouring under symptoms of stone in the bladder, and which had annoyed him more or less for two years previously, consulted me in April, 1864. He was in good health otherwise, and had been of active habits, fond of shooting and coursing.

^a Since writing the above I have perused a practical paper on Lithotrity, by my friend, Dr. Wilmot, in the November number of this Journal, 1852.

At times he suffered a little from piles, and at first attributed his uneasiness to them. About twelve months before he came under my notice, he felt that driving gave him pain in the glans penis, and that he was also unable to walk as well as usual in consequence of irritability of the bladder. He had never passed any bloody urine, nor had he felt excruciating pain when his bladder was emptied. He was seen by Mr. Adams, in consultation with me, and when we sounded him we quickly ascertained the presence of a stone. His prostate was not enlarged; and although I did not measure the calculus, the passage of the sound over the foreign body gave the sensation as if it were not large. His urethra was full-sized and healthy, and his urine was highly acid. Deeming the case favourable for breaking the stone, I advised the operation. Having business of importance, however, to transact in the country, all treatment was postponed until the end of the following month, when he placed himself under my care. On the 3rd of June, assisted by my friend Mr. Wharton, I proceeded to perform lithotrity in the following manner:—I laid him on a sofa, with a thick pillow under his buttocks, which I thus raised about two inches above the level of his shoulders, his head meanwhile being supported by another pillow. I then injected about six ounces of tepid water into the bladder. Having then warmed (by dipping it suddenly into a vessel of hot water) the powerful fenestrated lithotrite of Weiss, improved by Mr. Henry Thompson, I oiled it well, and slowly introduced it. As the instrument passed across the bladder, I distinctly felt it strike against the stone which lay to my left as I stood between his legs. Cautiously turning the beak of the lithotrite a little to the opposite side, I pushed it towards the posterior part of the viscus, in order to prevent the male blade, whilst opening, from displacing the calculus. Having then gently opened the blades, I turned them down on the stone, and fortunately seized it. The scale on the instrument indicated its size, which was three-quarters of an inch in diameter. I then steadily screwed home the male blade, when the stone was heard to give way with an audible crack. Opening the jaws a second time, and turning them, thus separated, to the left, I caught a fragment, and shattered it in the same manner. A third piece was seized similarly, and similarly crushed. Three minutes and a-half having elapsed during the operation, I withdrew the lithotrite, and had the patient gently lifted into bed, and placed a quarter grain morphine suppository in his rectum. Throughout the entire procedure he did not complain,

and described the sensation of the instrument moving in his bladder as causing more uneasiness than actual pain. I desired him to retain his urine as long as possible; and when obliged to void it, to do so whilst lying on his back. I requested him also to drink freely of barley-water, and to have some beef-tea two hours after the operation. I saw him three hours after, and found him easy. He had passed water slightly tinged with blood, and containing two little clots about the size of small peas. Four or five minute fragments had come away also, not larger than a pin's head, but showing the composition of the stone (lithate). They were caught in a little net of fine muslin, which I had placed over a small urinal, in order to collect detritus when he made water. I visited him again at 9 o'clock, p.m., the day of the operation, and he was suffering from retention of urine, arising evidently from the swelling of the neck of the bladder in consequence of the irritation of the lithotrite. I drew off, on this occasion, a large quantity of urine, which was merely coloured with blood, and thus gave him great relief. I introduced, as before, a morphine suppository.

June the 4th.—I saw him at 8 o'clock, a.m. He had passed a comfortable night, slept well, and emptied his bladder twice, but each time with some little delay and difficulty. No fragments passed. I kept him in bed this day, and ordered him to continue the beef-tea, and have one glass of sherry, as he was not feverish nor in pain.

June the 5th.—Three fragments of considerable size came away during acts of micturition. One of them was arrested at the orifice of the urethra for an instant or two, but passed out at last with the current of water.

June 12th.—A second sitting. This day I used the flat-bladed lithotrite, recommended by Thompson, to act on the fragments, and operated as he lay across the bed, with a bolster placed under his nates. I caught four pieces of the stone and broke them up, and then withdrew the instrument, which brought out in the female blade a good deal of pulverized calculus. He passed water before I left him; it was not bloody, but contained two or three minute clots, and a good deal of fine detritus was washed out with it. It would be tedious to record the further progress of this case from day to day. It will be sufficient to add that he suffered little or no pain throughout the entire treatment; and after having submitted to eight sittings, was perfectly freed from the complaint. An interval of nine days elapsed between the first and second sittings, and even longer periods intervened between the other operations,

as he frequently requested me to allow him to postpone them. He left town in the middle of September, about a fortnight after I had pronounced that his bladder was free from detritus, and all symptoms of irritation had subsided. I have lately heard from this gentleman, and he informs me that he is in the best of health, and able to resume his amusement of coursing, &c.

CASE II.—*Patient aged Thirty-six Years; Oxalate of Lime Calculus, of Small Size; Lithotrity; Recovery.*—Mr. A., an engineer, aged thirty-six years, consulted me on the 9th of August, 1865, suffering from symptoms of stone which had existed five months. He had taken a great deal of medicine to relieve what he was informed was an irritable bladder, but without relief. He was disturbed frequently at night to pass water, and latterly, after any exertion, the urine had a smoky appearance. He was excessively nervous, and refused at first to allow me to use a sound; but two or three days subsequently permitted me to do so. I introduced Henry Thompson's sound, a most useful instrument, I may remark, and one by which the surgeon can either draw off urine or inject the bladder without withdrawing it. For a moment I failed to detect any calculus; but, having allowed some of the water to escape through the sound, I struck a stone, which gave me the sensation as if it were very small. Having informed him of its presence, I desired him to return to his lodgings, to go to bed for two or three hours, and to take a draught containing two grains of quinine with fifteen drops of laudanum. I passed a No. 12 metallic bougie every third day for a fortnight, and by this means lowered the sensibility of the urethra.

August 26th.—I introduced the flat-bladed lithotrite for the purpose of measuring the stone, and of breaking it in case it was small. After about a minute I seized the calculus; the index on the handle of the instrument showed that it was not quite a quarter of an inch in the diameter in which I held it. I screwed home the male blade, and crushed the stone. I attempted to grasp a second piece, but failed. I then withdrew the lithotrite, in the female blade of which some detritus remained, and found it was oxalate of lime. The patient did not pass water for three-quarters of an hour afterwards; it was then bloody, and contained some little sharp fragments, weighing together four grains. The case progressed most favourably from day to day. Detritus was passed for five days in very small quantities, and then all symptoms of the

complaint disappeared. He remained in town until October, when I carefully sounded him, but could not discover the least trace of stone remaining. The lithotrite was used to sound him, but he never required a second operation, and has remained in excellent health ever since.

CASE III.—Patient aged Seventy-four Years ; Lithate Calculus, of Small Size ; Lithotrity ; Recovery.—John M., a farmer, aged seventy-four years, was sent to me by my friend, Dr. M'Evoy, of Balbriggan, and admitted into the Meath Hospital, October 22nd, 1865. He felt irritation in the bladder two years previously, shortly after recovering from an attack of rheumatic fever. He suffered from pain in the glans penis after passing water; and for the six months prior to admission his urine was always more or less tinged with blood, particularly after walking or driving. He was also greatly annoyed by tenesmus and diarrhea, his bowels being moved as often as five times in the twenty-four hours. He applied to Dr. M'Evoy, who at once suspected from the symptoms that he had a stone in his bladder. I sounded him, and struck the calculus.

October 23rd.—His urethra being large, I measured the stone with a flat-bladed lithotrite; the diameter in which I caught it was $\frac{9}{16}$ ths of an inch. His prostate was enlarged, which rendered the introduction of the instrument somewhat tedious. He was unable, it may be added, to retain his urine for more than one hour at a time, but there was no evidence of kidney disease.

October 27th.—He was brought into the operating theatre of the hospital, and as the size of the stone did not require a very powerful lithotrite, I crushed it twice with the flat-bladed instrument, before many of my professional brethren, who favoured me with their presence, and a large class of students. The patient, shortly after the lithotrite was withdrawn, made water, which was bloody, and contained a few small fragments of the stone (lithate). I placed a morphine suppository in his rectum, and had him carried to his bed, which had been previously well warmed.

October 29th.—Some large pieces of detritus came away.

On November 1st the second sitting took place, three fragments were broken, and a considerable quantity of pulverized calculus was brought away in the female blade of the lithotrite.

November the 4th was the day for the third sitting. The time occupied on this occasion was only two minutes, as the patient became weak and oppressed in his breathing; only one fragment

was caught and crushed. The patient got some warm brandy and water. There was no blood in the urine, which he discharged almost immediately after the removal of the lithotrite.

November the 7th.—His left testicle became very much swollen and dreadfully painful, but the symptoms of vesical irritation had almost completely subsided. Four leeches were applied to his scrotum, succeeded by a linseed-meal poultice.

November 9th.—The swelled testicle was not relieved. Several fragments had passed along with the urine between this date and the last sitting.

November 11th.—He was attacked with bronchitis, and his testicle was much worse—evidently advancing to suppuration. All operative measures, with respect to the calculus, were now suspended, and the treatment was altogether directed towards the chest affection and the testis.

November the 26th.—I opened an abscess in his left testicle which gave great relief.

December the 12th.—The fourth and last sitting took place this day. One small fragment was seized and broken—the only one which remained of the stone which, in three days subsequently had completely come away. Every symptom of irritation in his bladder disappeared, he passed water with the greatest freedom on all occasions, and the most careful sounding could not detect the smallest foreign body in his bladder. All through the treatment of this case the patient was sustained with generous diet, and a liberal allowance of wine and brandy, and at the same time his nervous system was soothed by the frequent administration of opium. No fragment was retarded *in transitu* from the bladder, and the operation of removing the stone was crowned with success, notwithstanding the untoward circumstances of the bronchitic attack, and the abscess of his testicle.

The points on which there are differences of opinion are mainly in reference to the after-management of the patient. I must not weary my readers by going through all the various steps of the operation, but shall merely observe that the introduction of the lithotrite requires care and dexterity. Any surgeon who has been in the habit of merely passing catheters or bougies will find it quite a different procedure to introduce the lithotrite. Its peculiar short curve might injure the upper wall of the urethra, if the shaft of the instrument were too suddenly depressed before the beak had been coaxed through the membranous portion. Regarding the

seizure of the calculus, I do not consider it wise for the operator to adhere undeviatingly to any fixed rule, for he will find that sometimes the stone can be caught by turning the jaws of the instrument on it, and at other times by depressing the opened lithotrite to the bottom of the bladder, so that inclined planes are formed, which cause the foreign body to roll into the instrument. I have myself always found it easier to catch fragments of a stone by bringing the lithotrite to them, than by bringing them to the lithotrite. I have always used Henry Thompson's modification of Weiss' lithotrite, and believe it cannot be excelled for lightness, strength, and finish. It can be manipulated as easily as a sound or catheter in the bladder; and when a stone is caught, by merely touching a little spring the action of the screw can be brought into immediate play. The firm hold given to the surgeon by the peculiar cylindrical handle (Mr. H. Thompson's suggestion) is also of the greatest importance. I cannot speak too highly of the flat-bladed lithotrite for grasping fragments. Its construction is such that it is almost impossible for it to catch a fold of the bladder, and equally difficult for it to avoid picking up a piece of stone. It is also sufficiently powerful to break calculi of a moderate size, and it pulverizes to a great extent the fragment it lays hold on.

Two of the greatest lithotritists of the present day, Sir Wm. Fergusson and Mr. Henry Thompson, differ in opinion respecting the removal of fragments after crushing. The former advocates taking them away by instrumental interference; the latter advises that they should be broken small enough, and permitted to be washed out by the urine. I certainly must pronounce in favour of the latter mode, for it would be impossible to bring out sharp and irregular pieces without more or less injury to the urethra, while the searching about in the bladder must be an unnecessary infliction of pain to the patient. The large sizes of the fragments which are passed is remarkable, and the various shapes which we should think most likely to become arrested in their course, as they travel through without a stop, excite wonder. The lithograph, Plate I., will assist in realizing this, as the fragments above and below the great mass of detritus are delineated in their natural shapes and dimensions, as they were passed by the gentleman, Case I., while it also shows the nucleus (*a*) as it was expelled. It is strange how small the advantage is of washing out the bladder. I have never seen any considerable amount of detritus brought away in this manner. Although I have frequently drenched the bladder, I

was never able to remove a fragment of any magnitude. A small quantity of fine detritus may occasionally be carried out in this way, but as a general rule the effect is *nil*. It is hard to say whether it may not be that the large eyes of the evacuating catheter do not come to the proper level to allow of the entrance of fragments, for I have frequently seen pieces passed by the patient that could easily have gone into and through the catheter, and after all were not removed by it. In the after-treatment of the patient it is a matter of the greatest importance to ascertain whether the bladder is *perfectly* freed from all traces of the stone. When vesical irritability has subsided, and even when a sound has been introduced and moved in every direction in the bladder without feeling any fragment, still some minute portions may remain without detection by the instrument. Of course freedom from pain, and less frequent calls to pass water, are strong proofs that all is right, but I have remarked that one of the best proofs of a complete cure is afforded when the patient is at *all times* able to make water in a large uninterrupted stream. I have noticed that when a piece is left behind, the patient may occasionally micturate with the most perfect ease and freedom, but at other times he finds he cannot make water for a moment or two, and that the stream, when it begins to flow, does not come with force. As long as a patient is in this condition I believe the surgeon cannot safely declare him *cured*; and I should advise him at this juncture to direct the patient to retain his water for about three-quarters of an hour (as a small quantity renders it easier to come in contact with fragments), and to introduce the flat-bladed lithotrite instead of a sound, so that by turning it in every direction the little offending piece may eventually be felt. It is often caught by turning the blades of the instrument downwards, and picking the fragment up whilst in this position.

As regards the constitutional treatment of the patient, a word or two may be added. The free administration of opium, in the form of a suppository, or given internally, is of the greatest advantage throughout the entire conduct of the case. Along with this I have seen much benefit accrue from a moderate allowance of wine. An aged patient is frequently observed to become disheartened and depressed, and may often be invigorated by the use of wine; not neglecting the opium, in order to relieve pain, procure sleep, and allay irritability of the nervous system.

ART. II.—*Notes in Medicine and Surgery*—IV. By PHILIP CRAMPTON SMYLY, M.D., Univ., Dub.; F.R.C.S.I.; L.C.P.I.; Surgeon to the Meath Hospital and Co. Dublin Infirmary, and to the Institution for Sick Children, Pitt-street.

1. A LARGE OVARIAN TUMOUR; OVARIOTOMY; RECOVERY.
2. TWO CASES OF STRANGULATED FEMORAL HERNIA, COMPLICATED WITH IRREDUCIBLE OMENTAL HERNIA, OF MANY YEARS STANDING; OPERATION TO RELIEVE THE STRANGULATION; OMENTUM CUT AWAY; RECOVERIES.
3. FORCIBLE FLEXION FOR ANCHYLOSIS OF THE KNEE-JOINT, OF TWO YEARS' STANDING; RECOVERY, WITH USEFUL LIMB.
4. COMPOUND FRACTURE OF THE CLAVICLE—SAID TO BE A VERY RARE ACCIDENT.
5. A MODE OF FIXING THE LARYNX WHILE OPERATING WITHIN ITS CAVITY, IN CONNEXION WITH LARYNGOSCOPY.

1.—OVARIOTOMY—RECOVERY.

“OVARIOTOMY is comparatively a new operation. Unlike lithotomy, herniotomy, or amputation, it wants the guidance, and is free from the trammels of tradition or long history. It is hardly fifty years since it was first performed; not forty since Lizars first attempted it in Great Britain; not twenty-five years since its first performance in a London hospital; there are still many hospitals in this kingdom in which it has never been performed; and there are some few men of eminence and authority in this country, and very many more abroad, who still regard it as an operation which is never justifiable under any circumstances.”^a

This being the case, it is the duty of every surgeon who performs the operation to lay before his brethren his experience as he gains it. Cases, simply related, when successful, may guide others to success; when unsuccessful, may save others from similar misfortunes.

It is needless for me to enter either into the history of the operation or the pathology of the disease.^b It is sufficient at present to say that Mr. Spencer Wells took up ovariectomy when the highest and best of our profession looked upon it as very little short of criminal to

^a Diseases of the Ovaries, by T. Spencer Wells. 1865.

^b See Dr. Ritchie—Ovarian Physiology and Pathology. London: 1865.

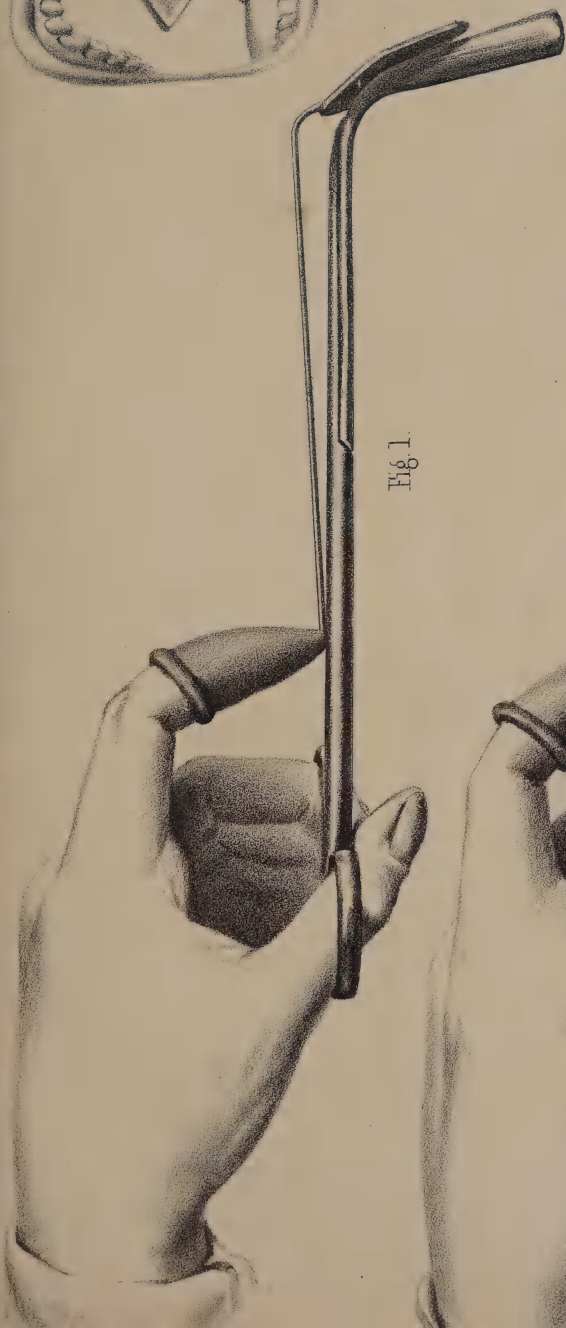


Fig 1.

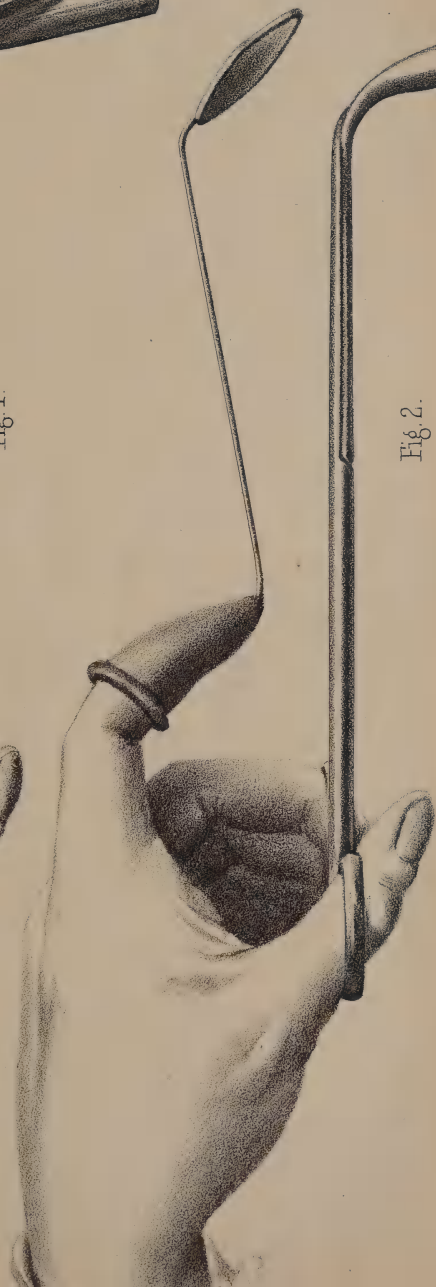


Fig 2.

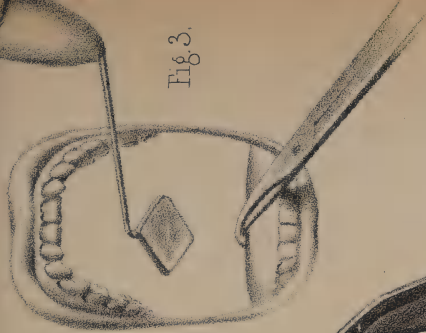


Fig 3.

attempt; and has raised it to even a more favourable position, with regard to the rate of mortality, than many of the operations hold that are sanctioned by daily usage.

CASE.—Miss S., aged forty.—About two years ago Miss S. had her attention called to the unusual size of her abdomen. She had never felt pain or distress until after a long and trying attendance on a relative, when she began to lose flesh and to look delicate.

Finding that the swelling of the abdomen was increasing, and that her feet and ankles also had swelled, she sought medical advice.

Diuretics, continued for a short time, completely removed the swelling of the lower extremities, but made no impression upon that of the abdomen. She was then given medicines, which she said were intended to purge her, but they produced violent vomiting. In spite of this active treatment the abdominal enlargement rapidly increased.

She then came to town, and I saw her shortly after her arrival. She was thin and pale, without appetite; bowels costive; had not made water for two days; the menses had ceased for three months, but had returned slightly on her arrival in town; and she had some swelling of the ankles.

Her abdomen was very large and tense, dull on percussion everywhere except just below the false ribs on either side. There was a distinct wave from side to side. The integuments, though very tightly stretched, could be freely moved and pinched up between the fingers over every part of the tumour. When the patient was directed to make an effort to sit up the recti muscles stood out, distinctly over the tumour.

The swelling was nearly symmetrical, slightly larger on the left side. Miss S. stated that she had never observed any difference, and she had no idea from which side the disease started.

Believing the case to be one suitable for operation, I called in Dr. Churchill and Mr. Porter, who saw her separately, and both came to the same conclusion, namely, that the disease was ovarian dropsy, and that the case was a favourable one for the great operation.

The family history was not very favourable. Her father died of kidney disease at ninety, her brother died of the same disease at forty-four, and one sister died of cancer at fifty.

Dr. Churchill made a very careful examination, and pronounced the uterus movable, and as far as he could judge, of a natural size. The urine was free from albumen and from deposits.

The dangers and advantages of ovariectomy, as compared with paracentesis, having been fully explained to the patient, she determined, after consulting with her friends, upon undergoing the operation.

On Saturday morning, the 18th of November, I operated, assisted by my colleagues, Mr. Porter, Mr. Collis, Mr. Wharton, and Mr. Rawdon Macnamara, and my friend Dr. Churchill.

Mr. Macnamara gave the chloroform; and I must here call attention to the great importance of having an experienced chloroformist for this operation. When the chloroform is carelessly given the operator is at one moment embarrassed by the convulsive movements—at another he is alarmed by the deep snoring of the patient. In the present instance the patient never moved, had no nausea, and knew nothing of the operation till some time after she had been settled in her bed.

The patient being dressed in a long flannel dressing gown, flannel drawers, and woollen stockings, was laid upon the operating table. The India rubber sheet recommended by Mr. Spencer Wells was adjusted to keep the patient clean and dry, and the chloroform administered.

An incision, about three inches in length, extending downwards to within two inches of the pubes, brought the linea alba into view. This was hooked up; a small opening was made to introduce a broad flat director, and was slit up to the full size of the first incision. Some layers of fascia were divided in succession in the same way, and finally the peritoneum.

The white shining surface of the tumour was thus brought into view. Mr. Wells' improved trocar and canula was then plunged into it, and the fluid allowed to escape through the India-rubber tube into a vessel under the table. As the sac gradually emptied it was drawn forwards with a vulsellum, and secured by means of two armed claws attached to the sides of the canula.

Gentle traction was made, and the sac came out without any trouble. As soon as the pedicle was brought into view, and it was ascertained that there was not much, if any, strain on the uterus, the clamp was firmly secured, the pedicle cut close to it, and the handles removed. Not a drop of blood oozed from the cut surface, though there were many large vessels in the pedicle.

The wound was closed with five silk sutures, passed from within through the peritoneum, muscles, and skin on either side. All the ligatures were introduced before any of them were tied; the advantage of this is that the wound may be opened again, and any drops of blood carefully sponged away, before the edges are finally brought together.

The clamp seems to me infinitely preferable to the cautery, which often necessitates dissecting out, and tying the vessels afterwards.

A small piece of lint was placed under each end of the clamp, and another over the wound. The India-rubber sheet was then removed, and the abdomen was covered with several layers of cotton wool, secured by long strips of adhesive plaster. A double flannel binder from the hips to the breasts, firmly applied, made all safe.

The patient was then gently lifted into her bed (which had been previously warmed) and was surrounded with hot water bottles. On recovering from the chloroform, she was given some hot beef-tea, with a tablespoonful of brandy.

The tumour consisted of a trilocular cyst—the largest of which contained seventeen and a half pints of thick brown fluid; the other two were small, one containing about a pint, the other about half a pint of fluid. The sac was not weighed, as it contained no solid matter whatever.

Dr. Barker, to whom I gave the sac for the College of Surgeons Museum, says, “The ovarian tumour which you sent to the College last month was remarkable for its smooth, glistening, and white surface, presenting no inflammatory action on its surface.”

In the evening her pulse was 120; she had been in a copious perspiration the whole day. At 9 o'clock, p.m., she got a draught containing thirty drops of liq. opii. sed. She got no refreshing sleep through the night. The whole of Sunday she was perfectly tranquil until about 2 o'clock on Monday morning, when the nurse sent for me, saying that Miss S. was in great pain. I found her in great distress, complaining of severe pains in the *stomach*, great thirst, and her pulse 150.

The pain I found was entirely due to flatulency, and was easily relieved by a little rubbing with the hand, without loosening any of the bandages. Finding that she had had no sleep, I sat down beside the bed to see if I could find out a cause. In the course of the first ten minutes the nurse woke the patient three times. She

was at once sent to bed, and I sat by Miss S. for the rest of the night. The patient slept for four hours and a half, and woke with her pulse 90, her countenance calm, and was altogether refreshed.

The nurse was at once dismissed, and I was fortunate enough to secure the services of Mrs. Farrell, who has been most efficient.

The urine was drawn off every three or four hours. It was clear and copious for the first forty-eight hours. On Monday it became somewhat turbid. On Tuesday it was less in quantity, with copious brick-dust deposit. On Wednesday (the 5th day) the first urine in the morning was very scanty, high-coloured, and with a great deal of deposit.

On Monday, Tuesday, and Wednesday there was considerable distress from flatulency—the whole abdomen became distended. On Tuesday there was some pain, which was relieved by three grains of opium, given in half-grain doses every hour or two. The flatulency was relieved by introducing a large catheter into the rectum from time to time, as suggested by Dr. O'Beirne^a in his valuable work on defecation; mustard, turpentine, &c., having completely failed to give any relief.

The sutures were all removed on the 23rd of November (the 6th day), and the wound was found completely united down to the edge of the clamp. The dry portion of the pedicle was then cut away, and the clamp opened and removed. Finding that the withered part of the pedicle, which had been pressed in the clamp, was inclined to retreat within the wound, I placed a ligature round it.

On the 25th her pulse was 90. There had been a fluid motion from the bowels through an India-rubber tube.^b The abdomen was quite flaccid, and there was no flatulency. On the 26th the urine was copious, without deposit, and of a natural colour, but had a very strong fetid smell, similar to that of the lint removed from the open part of the wound. A carrot poultice applied to the wound, and frequently changed, in about twelve hours completely removed the smell.

On the 29th the urine again had the putrid smell, which was corrected by applying a lotion of Condy's disinfecting fluid to the wound. On the 4th of December the rest of the pedicle came

^a It was rarely necessary to pass the tube beyond the sphincter ani, except on one or two occasions, when it was necessary to pass it up about six inches.

^b The tube alluded to in the text is a simple India-rubber tube, with the nozzle of an enema machine attached to it. It was found very useful for allowing the flatulency to escape, and saved the bed from being soiled.

away. She was ordered a teaspoonful of castor oil, as the bowels had not been moved for ten days. Two hours after the medicine acted, bringing away a quantity of hardened feces.

Dec. 12.—The wound was almost quite healed, and Miss S. sat up for four or five hours. The bowels acted without medicine nearly every day.

On the 14th she walked about the bed-room with a little assistance, and sat up the whole day. She had no sensation whatever in any part of the abdomen.

Dec. 16.—She was brought down to the parlour; and on the 18th of December, four weeks and three days after the operation, she took her first drive. She walked to the carriage, drove for more than two hours, walked from it again into the house, and suffered no uneasiness whatever.

2.—TWO CASES OF STRANGULATED FEMORAL HERNIA, ETC.

The two following cases occurred within a few weeks of each other, one in private and one at the hospital. The similarity of the two cases is very curious. In both the strangulation of intestines had been over three days. In both the omentum had been irreducible for many years (for eight in one case, nine in the other). The only material difference between the two cases was that the sac in one was a mere membrane, in the other it was very thick and dense.

CASE I.—Mrs. T., aged sixty-five, was taken ill on Monday, July 31, 1865, with violent pain in the stomach and bowels, and with violent vomiting. Dr. — gave her saline draughts for two days, but without giving her any relief.

On the following Wednesday evening I saw her, and found her vomiting everything; she had had no evacuation from the bowels since Monday. Suspecting strangulated hernia, I insisted upon an examination.

In the left groin I found a tumour about five inches in length and about four inches across; two above and two below Poupart's ligament. It was firm and dense, except at the inner side next the pubes, where it was springy, soft, and tympanitic. Perceiving that I was carefully examining the swelling, she said:—"That has been there for more than nine years, and has not been either larger or smaller for a very long time, and has nothing to do with the present attack." I tried taxis for some time and the warm bath, but

without effecting any reduction; and as the latter had given some relief, and the vomiting had ceased, I left her until morning, with a linseed poultice over the tumour.

In the morning, finding that the vomiting had returned, and the tympanitic portions of the tumour had not altered, I determined to operate.

Assisted by Mr. Porter, I operated on Thursday morning (the fourth day of strangulation).

A single incision was made across the tumour, over the femoral ring, about four inches long. The fasciæ were divided, and the sac was opened as usual, and the stricture divided directly upwards with Weiss' hernia knife.

The strangulated intestine was reduced with some difficulty. The omentum was firmly attached to the ring, so that reduction was out of the question. I therefore ligatured the mass close to the ring with a strong silk cord, and cut the whole away close to the ligature (about five ounces).

The wound was closed with two points of wire suture. She was ordered half a grain of opium every hour. The vomiting ceased immediately after the intestine was reduced. The bowels were freely moved two hours after the operation, and several times in the course of the day.

She had not a single bad symptom afterwards. The ligature round the stump of the omentum came away on the twelfth day, and the bit of slough came away next day. On the fifteenth day after the operation Mrs. T. was up and walking about. I got her a well-fitting double truss, and she is now able to walk as well as ever, and is in perfect health.

Case II.—reported by Mr. Duke, Resident Pupil.—“Margaret O'S., aged forty-five (more likely sixty), married, and the mother of four children, was admitted into the Meath Hospital about twelve o'clock on Tuesday, September 26th, suffering from strangulated femoral hernia in the right side.

“About eight years ago she had a bad cold; and, after a severe attack of coughing, she felt a ‘little lump’ in her groin, but did not know what it was until six months afterwards, when she went to one of the surgeons of Mercer's Hospital. He told her what was the matter, and ordered her to wear a truss, but she was never able to do so, because it ‘used to cut her.’

“She was always able partially to reduce the hernia until Sunday last, when she had eaten ‘a heavy dinner of cabbage, and taken-a

couple of glasses of porter.' The same evening she was attacked with severe vomiting, which continued all that night and next day. Tuesday morning she called in Dr. Moore, who sent her into hospital.

"Mr. Smyly saw her shortly after; and, finding reduction impracticable, operated at four o'clock."

The operation was as follows:—The skin and fasciæ were divided in the usual way, by a perpendicular incision, and the sac exposed. The stricture was then divided, and some intestine reduced; but, as there was much omentum that could not be reduced, it was thought better to lay open the sac. Some hardened omentum was found firmly adherent round the neck of the sac. This was securely ligatured and cut away (3i. 3ii.ss.). As the opened sac then looked large, and its walls were very thick, it was thought better to remove some of it. This was done by cutting, with a pair of scissors, about quarter of an inch from the opening of the ring; the wound in the skin was then closed with three points of wire suture. Report continued:—

"The patient was then placed in bed, and was given small quantities of wine and brandy, and half a grain of opium every hour. Her pulse, after the operation, was 160, and rather feeble.

"Wednesday 27th.—Slept well last night, having taken five pills; tongue, white; pulse, 140; bowels not moved; ordered an enema, which had no effect; another in the middle of the day, and a third in the evening; the latter acted pretty well.

"Thursday 28th.—Slept a good deal through yesterday, and not so well last night; slight discharge from the wound; two of the sutures taken away; tongue same as yesterday; pulse, 123 in the morning, and in the evening 118 in the minute; no motion from the bowels; ordered a cough bottle, as she has a very troublesome cough.

"Friday 29th.—The patient had a good night, and the bowels were relieved this morning; pulse, 112; the remaining suture was taken out this morning; some discharge from the wound; the same dressing continued—a pledget of wet lint, and a towel laid over it; the lint was changed morning and evening.

"Saturday 30th.—Her tongue was cleaner this morning; pulse, 100.

"From this the patient improved steadily. The ligature round

the omentum came away on the 13th of October. She left the hospital quite well the first week in December."

3.—FORCIBLE FLEXION OF THE KNEE FOR ANCHYLOSIS IN THE STRAIGHT POSITION.

In February, 1863, Miss W—— came to me with an introduction from my friend, Dr. Smith, of Borris-in-Ossory.

She had suffered from a violent attack of rheumatic arthritis some years before, when abroad. The limb had been fixed in the straight position, and had become perfectly ankylosed. There was not the slightest motion in any direction, and the patella was firmly attached. She had had no pain for a very long time; and there was not the least evidence of any present inflammatory action. The measurement of the affected knee exceeded that of the healthy one by about an inch and a-half.

Miss W—— complained bitterly of the constant source of vexation her stiff knee was to her. She could not walk any distance on account of the fatigue caused by swinging the limb round to place it in front of the other; she could not sit down with comfort except on a very high seat; she could not kneel; and she could get up stairs only by "putting her best foot foremost," and dragging the stiff one after her.

Under these circumstances I advised her to have the joint forcibly flexed, and then to employ passive motion.

I found that my views coincided with those of my friend, Dr. Smith. Subsequently Mr. Collis saw Miss W——, in consultation, and he also considered the case a favourable one for operation.

Assisted by Dr. Smith and Mr. Collis, I operated as follows:—

I first bandaged two strong iron splints, well padded with cotton wool, to the thigh—one in front and one behind—extending from about half-an-inch above the point I wished to effect the fracture to the groin in front, and as far behind. This was to avoid the danger of breaking the thigh bone instead of the knee.^a Miss W—— was fully chloroformed and drawn down on the table, so that the end of the posterior iron splint rested exactly on the edge. Mr. Collis with both hands propped firmly on the anterior splint.

I grasped the foot with my right and placed my left hand about

^a On two occasions I was present when femur gave way in the middle—one was forcible extension, the other forcible flexion.

the middle of the tibia. By throwing the whole weight of my body on my left hand I succeeded in breaking through some of the adhesions. This process had to be repeated three times before the knee could be bent to a right angle. Each time the adhesions gave way with a loud crunching sound. The thigh splints were then removed, and the limb placed in a long jointed splint, with a screw behind, and lightly bandaged—the leg and thigh being left at an angle of about forty-five degrees.

The knee was kept constantly iced for forty-eight hours, and the foot kept warm.

Every day the position was slightly changed, so that towards the end of the week the limb was again straight. The screw was then slowly tightened, several times a day, until, in about three days, the leg and thigh were nearly at a right angle. For about six weeks this gradual process of flexion and extension was persevered in; then the splint was removed, and Miss W—— was allowed to get up. She was desired, always, to sit on a low chair and to try and strain the foot backwards so as to touch the leg of the chair.

Miss W—— then returned to the country and has enjoyed perfect health ever since. Her present state I cannot better describe than by giving the following extracts from Dr. Smith's letters, dated November 22nd and 28th, 1865, nearly two years after the operation:—

“MY DEAR SIR,—You may make what use you please of my name in reference to Miss W——'s case in publishing your report of it.

“About two years before Miss W—— consulted me she had an attack, while in France, of what I took to be rheumatic arthritis of the affected joint, which ran a severe course, the limb ultimately being put up, by her medical advisers, with the view to establish a stiff joint as the best result to be expected, which probably at that time was sound practice enough. However, at the end of two years, all inflammatory tendency having subsided, I considered it a suitable case for forcible flexure, and with the object of putting my views in force, called in two medical men in the country, one after the other, to assist me in the operation—one, a man of very large reputation—but both positively refused, fearing results; and Miss W—— was about to abandon all thoughts of ever having the use of her limb restored, were it not that I still maintained my original opinion, and insisted on her having further advice. I first sent her

to Dr. — who kindly examined the case for me, and coincided with me in the opinion that it was a case for operation. However, I thought he showed a reluctance to enter into it. I then asked you to see her, and you at once entered most kindly, heart and hand, into it. What followed is so well known to you as to require no comment from me further than to say—I saw Miss W—— very lately, and a more complete and perfect success could not follow in any such case. In fact, she has as good use of that knee as of the other—can take long walks, dance, run, or exercise in every way as freely as before the joint became affected.

“I remain, yours faithfully,

“HENRY J. SMITH.”

4.—COMPOUND FRACTURE OF THE CLAVICLE; RECOVERY.

Compound fracture of the clavicle is a very rare accident, though from the closeness of the bone to the skin its frequent occurrence might be expected.

The following notice of the accident may be found in Professor Hamilton's valuable work on fractures, published in 1860:—

“I have never seen a compound fracture of this bone, although in many cases the sharp point of an oblique fracture has seemed just ready to penetrate the skin.

“One case is reported as having been presented at St. Bartholomew's Hospital. It occurred in a boy, fourteen years' old, and was produced by his having been drawn into some machinery while in motion. Seute also mentions a case, seen by himself, occasioned by the fall of a derrick upon the shoulders. . . .

“Ayres mentions another case, the result of a severe gunshot accident, in which the bone was also much comminuted.”

The three cases described by Hamilton were all the result of direct violence. The following case I believe was caused by *contre-coup*:—

CASE, reported by Mr. Percival, resident pupil.—“Bridget R., aged sixty, was admitted into the Meath Hospital, between one and two o'clock on the morning of the 6th of August, 1865. She stated that as she was stepping from the footpath to cross the street, she was seized round the neck by a man and thrown down with great violence to the ground. She fell on her left shoulder,

and *heard* the bone crack. She vomited immediately after the accident. When she arrived at the hospital about three quarters of an inch of the acromial end of the clavicle was found protruding from a wound in the skin corresponding to the middle of the bone."^a

Mr. Percival sent for me at once, being on accident duty at the time.

The patient was lying in bed, Mr. Percival having reduced the displaced bone, and secured the arm in the usual position for fractured clavicle.

I found a lacerated wound, corresponding to the fracture, large enough to receive the point of the little finger. On sponging away the blood, which oozed slowly from the wound, I ascertained that the fracture distinctly communicated with it. A compress of lint was placed over the wound, and the fracture was treated as a simple one. She was discharged, well, in six weeks.

5.—A MODE OF FIXING THE LARYNX.

"Necessity is the mother of invention." I know not whether any fellow-labourer in the field of laryngoscopy has hit upon the plan about to be described for supplying the want of a third hand—a deficiency often much felt by the operating surgeon.

A case lately came under my observation of numerous small polypi in the larynx, at the base of the epiglottis. The patient was tongue-tied, had a very large tongue, an epiglottis that almost touched the back of the pharynx, even during phonation, and the larynx very deeply set in the throat. The practical laryngoscopist can alone realize what difficulties these were.

At last I hit upon the following plan :—Mr. Fannin got a forceps made for me with broad blades, bent as shown in the lithograph, to dilate the larynx and raise the epiglottis. This is held by the thumb being passed through one ring, the little finger through the other. The second and third fingers rest on the side to steady the instrument.

The stem of an ordinary faucial mirror, out of one of Weiss' cases, is bent, and the bent part beaten out flat, to prevent it turning on the finger. This is attached to the index finger by an India rubber nipple. Thus the left hand is armed with a dilator and mirror, leaving the right hand free for action.

^a The policeman swore to two inches on the trial.

Fig. 1 shows how the hand should be held before the dilator and mirror are introduced.

Fig. 2 shows them when introduced, the mirror being raised against the palate.

Fig. 3 shows the view the operator gets of the points of his dilator and the field for action between them.

ART. III.—*On Typhoid Pneumonia Associated with Muffled Tympanitic Resonance, with Cases.* By THOMAS HAYDEN, F.R.C.S.I.; L.K. & Q.C.P.I.; M.R.I.A.; Physician to the Mater Misericordiæ Hospital.

IN the course of the past Winter I have had under treatment in hospital several cases of pneumonia, two of which possess considerable interest, as presenting features of an unusual character and great difficulty in the interpretation.

I will submit these cases in detail, together with two others which in some measure illustrate them, with such remarks as may seem applicable.

Typhoid Pneumonia Engaging the Superior Lobe of the Left Lung, in which Tympanitic Resonance was Developed on the Fourteenth Day of Illness, Successfully Treated with Alcohol and Small Doses of Quinine.—Sylvester Dempsey, aged forty-six years, a labourer employed in sulphuric acid works, where he was engaged in making "salt cake," was admitted into hospital May 24, 1865. On the preceding Saturday (May 20) he was attacked with pain in the left side, and on the following Monday was forced to relinquish work. On admission the skin was hot; tongue dry; pulse 108, full and soft; teasing cough, attended with pain in left side; and expectoration of rust-coloured sputa; upper and anterior portion of left side perfectly dull, and without respiratory murmur, or other respiratory sound of any kind; infero-anterior, lateral, and whole of posterior surface clear, as likewise was right side, and yielding respiratory murmur throughout. To be cupped to three ounces under left clavicle, and have one grain of sulphate of quinine, with four minims of sulphuric ether, and two of dilute sulphuric acid in water every third hour; strong beef-tea *ad libitum*, and three ounces of wine daily.

May 28.—Inflammation has extended downwards, engaging the entire of the anterior surface of the lung, as low as the sixth rib; left side of chest generally dull in front, to a point below the nipple, but clear behind a line extending obliquely downwards and forwards from the posterior fold of the axilla; pulse 90 and occasionally intermitting; heart's action corresponding, but otherwise normal; sputa "rusty;" tongue dry; features haggard and shrunk; to be cupped to three ounces in mammary region, and have, within the next twenty-four hours, ten ounces of whiskey and six of wine at intervals; quinine and beef-tea, with arrowroot, to be continued.

May 29.—Respirations 30; pulse 72, *not* intermitting.

June 1.—Progressing favourably; tongue now clean; pulse 72, full, regular, and not intermitting; respirations 30; returning vesicular breathing in apex of lung, with feeble and sparse crepitus. In mammary region bronchial respiration with muco-crepitus; general dulness in front; resonance behind, as on the 28th of May.

June 3.—Tongue clean; pulse 72; respirations 24; dulness in front less pronounced; in sub-clavicular region it is modified by a peculiar resonance of a somewhat tympanitic character, but from the clavicle to the sixth rib a crepitus is still audible, beneath the clavicle only at end of full respiration. Treatment continued, with exception of whiskey which was reduced to five ounces.

June 5.—Pulse 66; respirations 18; a coarse systolic murmur of a rubbing character, heard in second left intercostal space, near the sternum.

June 6.—Pulse 60; respirations 18.

June 8.—Pulse and respiration as on the 6th; tongue clean. A loud grating friction both heard and *felt* over base of left lung posteriorly, and bronchial respiration in left supra-spinal fossa.

June 12.—Pulse 60, full, strong, and regular; first sound of heart rather prolonged, and attended with a soft murmur over left base; whole of left side comparatively dull in front, with muco-crepitus inferiorly; pleuritic friction audible over greater part of same side posteriorly; appetite good; sleeps well. Discharged this day.

This was a case of typhoid pneumonia engaging the superior lobe of the left lung, and complicated, towards the close, with pleuritis without effusion, and a slight attack of pericarditis. The work in which the man had been employed was well calculated to deteriorate his health.

The inflammation, as is usual in typhoid pneumonia, invaded first

the apex of the lung, and then slowly travelled downwards, involving, however, only the superior lobe, and at no period extending to the posterior-inferior portion, or inferior lobe of the lung. Are we to seek, in this latter circumstance, an explanation of the tympanitic resonance, or rather that remarkable modification of dulness which might be expressed by the designation of "resonant dulness," which became developed on the 3rd of June, *i.e.*, thirteen days after the commencement of the attack? According to Skoda the answer to this question should be in the affirmative; as he holds that the presence of a substratum of healthy lung, freely permeated by air, is capable of communicating a modified resonance of a somewhat muffled character to a solidified portion interposed between it and the chest-wall subjected to percussion. On theoretic grounds this explanation might be deemed satisfactory. There can be no doubt that the presence of a stratum of healthy lung tissue, freely permeated by air, subjacent to a solidified and non-resonant portion, does modify in a marked degree the dulness of the latter, the percussion note yielded by which is of a compound character, representing, in the proportions in which they are relatively present, the two elements of which it is composed.

It will appear, however, on reference to Case No. II., that the phenomenon of modified tympanitic resonance in a solidified lung cannot be attributed to this cause exclusively; in that case, although the entire lung was hepatized the phenomenon was developed even in a higher degree than in the case now under consideration; whilst in Case No. III. all the conditions required by Skoda's doctrine for the production of resonant dulness, namely—a solidified lobe overlying one in a healthy condition were supplied, yet the phenomenon did not exist.

From these cases, therefore, the conclusion to be arrived at would seem to be, that although in a partially hepatized lung in which the solidified portion has a substratum of healthy pulmonary tissue, the percussion-dulness of the former may be modified by the resonance of the latter, in such a manner as to give rise to a hybrid sound of a very remarkable character; this phenomenon may be present where the entire lung has become solidified; and again, that it may be absent where a healthy and a solidified portion of lung-tissue occupy the relative positions mentioned.

The absence of respiratory sound, both vesicular and bronchial, noticed on the 24th May, over a region in which dulness was complete, deserves a moment's attention.

According to Dr. Stokes^a the absence of bronchial respiration in a solidified lung, without liquid effusion, should be accounted for by fixity or non-expansion of the side, resulting from engagement of the entire lung.

Grisolle^b admits the occasional absence of crepitus and "all respiratory sound" in the second stage of pneumonia, and he has made some observations to show that this is due to softening or defluence of the lung, by which the tubes are closed as a consequence of collapse of their walls; he states, moreover, that he has met with three cases in which the entire lung was solidified, as proved by *post mortem* examination, and yet bronchial respiration continued up to the time of death.

In the case of Dempsey one lobe of the lung only was hepatized, still both bronchial and vesicular breathing were temporarily absent in the affected portion; and its subsequent history, ending in recovery, suffices to exclude the assumption of softening or defluence in the sense in which it is put forward as an explanation by Grisolle.

If the negative phenomenon of total suspension of breathing-sound, associated with percussion dulness and the general symptoms of pneumonia, had been presented in the base of the lung where pneumonia is most frequently met with, the diagnosis of pleuropneumonia with effusion would probably, according to accepted rules, have been arrived at; and yet that diagnosis would have been incorrect in one particular. No doubt the treatment would have been all the same, and the diagnosis corrected on the following day, still the latter would have been, *pro tanto*, erroneous in the first instance.

On the 28th of May the patient's condition was most unpromising, as indicated by shrunk features, dry tongue, and a weak and intermitting pulse. The favourable change noticed on the following day must undoubtedly be placed to the account of the whiskey (3x) given within the preceding twenty-four hours; the pulse had come down to 72; now no longer intermitted; and the patient's general condition was perceptibly better.

The late Dr. Todd^c declared, "in the treatment of pneumonia I do not consider the administration of alcohol essential, but rather regard it in the light of an important accessory." It is doubtful,

^a Diseases of the Chest, p. 326.

^b *Traité Pratique de la Pneumonie.*

^c Clinical Lectures. By Beale.

however, that in the treatment of typhoid pneumonia at least, a substitute, and of equal efficacy, can be found for alcohol.

As regards the administration of sulphate of quinine in the treatment of pneumonia, first recommended by Dr. Corrigan,^a in the asthenic form of the disease, I cannot form an estimate of its value from the few cases I have treated with it. In no instance have I given it in the liberal doses recommended by Dr. Corrigan, and in which he so successfully administered it. I have not exceeded three grain doses, in which quantity it was given every third hour in Case No. III.

Dr. Flint^b has given sulphate of quinine in thirty-three cases of pneumonia, of which three only were fatal. The general result has been, in his practice, a notable decrease in the rate of the pulse and respiration.

As to the specific action of quinine in pneumonia Dr. Corrigan has given what seems the most rational view of its operation, namely—that in the first stage of pneumonia, when the pulmonary capillaries are gorged with blood as the result of temporary paralysis of the vaso-motor nerves supplied to them, quinine acts beneficially by stimulating the latter, and thereby diminishing the volume of blood circulating through the affected lung. This is, undoubtedly, the *modus curandi* in enlargement of the spleen.

CASE II.—*Pneumonia, with Tympanitic Resonance of the Right Lung; Death.*—James Osborne, aged twenty-six years, of intemperate habits, was admitted into hospital June 9th, 1865. The patient had been in a state of intoxication for several days preceding the above date, so that it was impossible to fix the period of commencement of his present illness.

On the day of admittance the following was his condition:—Pulse 120, sharp and full; respiration 54; skin hot and dry; face pale; unable to make full inspiration owing to pain in right side; dulness considerable, but not complete, over base of right lung both in front and behind. With this exception the right, and the whole of the left lung were normal as regards percussion. Over the base of right lung, to an extent corresponding with that of dulness, respiration was scarcely audible. There was no crepitus or friction to be heard. Scarcely any cough. No characteristic sputum.

^a Dublin Hospital Gazette, Vol. iii., July 15, 1856.

^b North American Medico-Chirurg. Review, March, 1861, and Half-yearly Abstract of Med. Science, Vol. xxxiii.

From the preceding symptoms and signs the diagnosis to be formed was manifestly acute pleuritis with rapid effusion; and this was the opinion given. To be cupped to three ounces over the right lung, and have two grains of calomel with two grains of antimonial powder every third hour.

June 10.—Fine crepitus detected in base of right lung. In other respects symptoms and signs as on yesterday. To be cupped again, and continue medicine. Diagnosis corrected.

June 13.—Dulness extends upwards to angle of scapula, and at all points below this level posteriorly, bronchial respiration and crepitus were audible; was occasionally delirious in the course of last night; pulse, 120, and full; respiration 54. Was bled from arm to seven ounces, viz., till the pulse began to fail. A poultice of linseed-meal and mustard applied over right side, and calomel and antimonial powder continued.

June 14.—No material change since yesterday. Gums exhibit the effects of the calomel, which was stopped. To have quinine ($1\frac{1}{2}$ -grain doses) and sulphuric ether mixture, and three ounces of wine; strong beef-tea *ad libitum*.

June 15.—Pulse 132, and weak; respiration 54; body bathed in cold sweat; jactitation and low muttering delirium. From base of thorax to right nipple there is complete dulness, with crepitus; from nipple to clavicle the percussion note is of a mixed and very singular character; it is that of dulness qualified by a metallic resonance, and communicates the sensation of a solid but resonant body under percussion. Over the region which presents this remarkable modification of dulness respiration is bronchial, accompanied by crepitus on full inspiration. To have one ounce of wine every hour.

June 16.—Pulse 114; respiration 42; tongue moist; otherwise no change.

June 17 and 18.—Whole of posterior surface of right side dull, and here respiration is bronchial.

June 19.—Pulse 150, and full; respiration 60; perspires copiously; has had several attacks of paroxysmal dyspnea since last report; tongue moist and coated in the centre with creamy fur; coughs occasionally, and expectorates viscid uncoloured mucus with much difficulty; right side of chest more prominent in front than left—this difference is more striking if infra-clavicular regions be compared; in this situation likewise, namely from clavicle to fourth rib, there is less respiratory movement on right side than on left;

right side measured under the nipple is nearly one inch in excess of left; a line carried from the apex of the right axilla downwards behind the nipple to the base of the thorax, and another line carried vertically from the fourchette of the sternum to the ensiform cartilage, slightly to the left of the mesial line, would define a space within which percussion-sound was tympanitic, and of a somewhat metallic character—less so immediately beneath the clavicle, and below the nipple, than in the intermediate space, viz., lower and greater portion of sub-clavicular, and upper portion of mammary region, where it was decidedly metallic; the heart pulsates feebly beneath fifth, sixth, and seventh costal cartilages immediately to the left of the sternum; in the course of last night had a paroxysm of dyspnea lasting one hour, which threatened suffocation, and during which his face became livid and his eye-balls prominent. To have four grains of quinine with ten minims of sulphuric ether, and two minims of diluted sulphuric acid in mixture every fourth hour. No hope entertained of patient's recovery.

June 19, 4.30, p.m.—Over entire of posterior surface of right side, which is perfectly dull, a fine crepitus is heard; over the resonant region in front respiration is bronchial, as it has been for the last three days, during which period likewise, and up to the present, a few crepitant râles are heard here when the patient makes a full inspiration after coughing. The left lung which had been resonant, and in which respiration had been hitherto puerile, yielded this morning dulness on percussion over the base, where coarse muco-crepitus was audible. Over both sides of the chest, throughout the patient's illness, vocal vibration was strong and equal, and at no period was there pneumonic sputum.

June 20.—Patient died at eight o'clock this morning. *Post mortem* examination three hours after death, as per notes taken at the time:—Body rigid; face pale; percussion yielded same result as before death, except that tympanitic resonance was a shade less pronounced on right side, which was remarkably prominent. The body was immersed, face upwards, in a leaden vat filled with water. The surface of the water was percussed by resting the dorsum of one finger horizontally upon it, and tapping gently upon the latter with the fingers of the other hand; over the left side of the chest the sound elicited presented nothing to arrest attention; but over the right side, which was placed about an inch and a half below the surface of the water, the sound yielded was decidedly tympanitic, and resembled that heard on percussing a leathern bag

filled with air. The right pleural cavity was now cautiously opened under water by an incision dividing the integument and muscles in the third intercostal space, in a line with the nipple, and by lacerating the costal pleura with the finger. The object of this proceeding was to determine whether the pleura contained free air. The finger was now introduced with some difficulty, owing to firm adhesion between the lung and costal pleura; a portion of this adhesion was broken down in the neighbourhood of the incision, and the lung detached to a slight extent from the chest-wall. The cavity of the pleura was thus laid open, *yet not a bubble of air escaped*. The surface of the lung was next pinched up with forceps through the incision, punctured with a scalpel, and lacerated with the point of the finger, when immediately *bubbles of air freely escaped*, and were witnessed by my colleague Dr. Cruise, and by the class.

The chest was now laid open in the usual manner; the pericardium was found distended with clear serum, and the anterior surface of the heart sparsely covered with minute but firm granulations of recently deposited lymph, more especially at the base, and over the root of the pulmonary artery. There was a good deal of fat in the usual situations on the anterior surface of the heart, which was firm, and of the normal size, or perhaps a little less. Both lungs were extensively adherent to the chest-walls, the right more firmly, and was likewise attached to the diaphragm throughout. The right lung was fully distended so as to keep that side of the chest in a state of maximum expansion; its colour was dark grey, and its anterior edge was thickened and overlapped the pericardium; the pulmonic fissures were obliterated by adhesion. The superior and middle lobes yielded on percussion a *somewhat muffled metallic ring*, a shade less clear than that yielded by the corresponding part of the patient's chest during the three last days of his life; the percussion-note elicited from the anterior edge of the lung *placed upon the hand* was similar, *i.e.*, muffled metallic. Percussion of the inferior lobe, at all points of its surface, yielded a *perfectly dull sound*.

A vertical section was made from apex to base of right lung, from its posterior thick edge in the direction forwards. The sensation communicated to the hand was that of a solid body under incision.

The cut surface was light grey in colour, somewhat darker at the base; no fluid escaped from it, but on pressure with the flat

surface of the knife thin purulent matter streamed out. Placed in water the lung sank at once.

The left lung was universally congested, the apex less so than the remainder. On section a thin frothy serum issued from it; it was resonant on percussion, and floated in water.

A photograph of the right lung has been taken, and copied in the subjoined woodcut by Browne and Nolan of Nassau-street.



Section of lung from behind.—*a a b b*, surface of section of superior lobe of right lung which had yielded tympanitic resonance; the solidification of the pulmonary tissue, and the divided blood-vessels are well represented. The deep congestion of the inferior portion of the lung is likewise seen.

The liver was somewhat larger than natural, and presented on its upper surface several large yellow blotches, not elevated above the surface or irregular; neither was the peritoneum thickened or become opaque over them. These blotches were found, on section, to pass into the substance of the liver a quarter of an inch. I regret I cannot present a report on their structure, examined microscopically, as the specimen intended for examination was unfortunately lost. With this exception the liver was apparently healthy. Other organs not examined.

I have thought it right to give, from my notes taken at the bedside, a detailed account of the progress of this case from day to day,

and likewise of the *post mortem* appearances, because of the interest that attaches to it in connexion with the phenomenon of tympanitic resonance in a solidified lung.

In Case No. I. this phenomenon likewise existed, but in a less degree, in a lung only partially solidified. In that case the hypothesis was at least warrantable, that the resonance yielded by the solidified portion was communicated from that which was in a healthy condition.

In the case just narrated, however, no ground existed for such an assumption, as the lung was hepatized throughout its entire extent. Neither was it admissible to suppose that the phenomenon was due to communicated gastric resonance, for, independently of the fact that it occurred on the right side, it will be remembered that it was manifested only in the upper portion of the lung, where it could not have existed, if transmitted from the stomach, without also manifesting itself in the inferior lobe. Lastly, percussion of the lung after removal from the body afforded convincing evidence that the resonance was intrinsic; a portion of it placed upon the hand, a solid body, and percussed in the usual manner, yielded the characteristic metallic note—resonance of a character similar to that heard during life over the corresponding portion of the chest.

The occurrence of *aëriform* accumulations, independently of rupture or perforation of the lung, in connexion with acute disease of the organs of respiration, is of great interest, as introducing an element of derangement into the rules of diagnosis of intra-thoracic disease.

These accumulations may be subdivided into two forms, both of which have been fully established, namely, those in the cavity of the pleura, constituting the “simple pneumothorax” of Graves,^a and the “secondary” form of pneumothorax without perforation of Jaccoud,^b and those in the substance of the lung itself.

In the two first cases given by Dr. Graves, there can be no reasonable doubt that there had been *aëriform* effusion into the cavity of the pleura. Of this, the displacement of the heart without liquid accumulation in the former case, and the compression of the lung without liquid distension of the pleura in the latter, afford convincing evidence.

The presence of a cavity in the apex of the lung in the latter case, in the absence of *positive proof* that no communication existed

^a Dublin Journal of Med., Vol. iv.

^b Gazette Hebdomadaire, Jan. 29, 1864.

between this and the pleura, may be taken as invalidating, in some degree, the evidence it affords in support of the doctrine of simple pneumothorax by secretion.

Of this form of gaseous secretion into the pleura another indubitable example has been admirably reported by Dr. Little of Sligo.^a

A case is mentioned by Dr. Stokes^b of typhoid pneumonia in a female, in which, on the eighth day of illness, the antero-superior portion of the left side, previously dull, yielded a sonorous tympanitic sound on percussion. On the following day resonance had extended to the postero-superior portion of the chest; but on the next succeeding it had disappeared and been replaced by dulness.

In this case, which is given as one of pneumothorax by Dr. Stokes, the character of the resonance described by him as being "similar to what is produced by the stomach in the highest degree of flatulent distention," was different from that observed in my cases, in which it was somewhat muffled.

That there is, however, another cause than effusion of air into the cavity of the pleura, whether by secretion or otherwise, to which resonance over a solidified lung may be due, the cases reported by Dr. Hudson, in his valuable memoir on typhoid pneumonia,^c afford strong evidence.

The cases now reported go to corroborate the evidence furnished by Dr. Hudson; and the second case carries that evidence a step further, by demonstrating *negatively* that no air existed in the cavity of the pleura, and *positively* that air did exist in the tissue of the lung, in that portion of the organ which yielded resonance both before and after death.

In Dr. Hudson's second case, as in mine, there was at first sharp pain, dulness on percussion, and absence of respiration and of crepitus over the affected portion of the lung—hence, in both cases, the erroneous diagnosis of pleuritis with effusion. Subsequently, the occurrence of bronchial respiration and of crepitus showed the error committed, and pointed out the real nature of the disease.

Morbid clearness was developed in the previously dull portion of the lung, in Dr. Hudson's second case, on the fourth day of illness; in my first case only on the fourteenth day, and in the second on

^a Case of "Simple Pleuritic Pneumothorax," Dublin Quart. Jour., Vol. xxxvi., Nov., 1863.

^b Disease of the Chest, Part i., page 334.

^c Dublin Journal, Vol. vii., July, 1835.

the sixth day. Of these three cases, it continued up to death in the two which were fatal, but in that which terminated in recovery resonance was replaced by dulness on the sixth day from its commencement. In all three cases *bronchial respiration* and *crepitus* coincided with *tympanitic dulness*.

It cannot be pretended that resonance was due to a central pneumonia in these cases; firstly, because of the *preëxistence* by several days of dulness in the resonant portion of the lung; and secondly, because *post mortem* examination in two of them, in each of which resonance continued up to death, showed that hepatization was *universal*.

In Dr. Hudson's seventh case bronchial respiration and crepitus likewise coincided with resonance, which continued up to death. Thus it would appear that morbid resonance developed in the progress of pneumonia may be associated with bronchial respiration and crepitus, as in the cases above cited; or with total absence of all respiratory sound, as shown by Dr. Graves; and the evidence before us would seem to warrant the conclusion, that upon this distinction may be based the differential diagnosis between resonance due to air included in the tissue of a hepatized lung, and that depending upon aëriform accumulation in the pleura.

Grisolle does not mention percussion-resonance at all in connexion with pneumonia; he only speaks^a of comparative dulness, and occasional absence of dulness in the pneumonia of children.

Dr. Stokes says^b:—"But of all these signs the most remarkable is tympanitic clearness over the diseased lung, a phenomenon evidently proceeding from an effusion of air by secretion into the serous cavity." Thus, it would appear that at the date of his great work (1837) this eminent physician, than whom nobody in our day has contributed more to the physical diagnosis of thoracic disease, was still unaware, or at least unconvinced, of the existence of the second form of pneumonic resonance, namely, that *not* depending upon pneumothorax.^c

Dr. Williams includes amongst the essential and characteristic

^a Opus citat, pp. 229 and 230.

^b Opus citat, p. 332.

^c Since writing the above I have had a conversation with Dr. Stokes, and ascertained his present views on the subject; he now fully admits a *pulmonary*, as distinct from a *pleural* form of tympanitic dulness in pneumonia; he regards it as a sign of formidable import, and believes that it is, when present, invariably associated with the typhoid type of the affection. This was certainly the character of the inflammation in one of my cases, but in the other it was of a more sthenic form

physical signs of pneumonia, "dulness of sound on percussion,"^a to the presence of which, in the advanced stages of the disease, he makes absolutely no exception, and nowhere mentions tympanitic resonance as of occasional occurrence.

Dr. Fuller in his work, *Diseases of the Chest* (1862), does not allude to tympanitic dulness in pneumonia, and in the two most recent works on the Practice of Medicine, namely, those of Trousseau^b and of Hughes Bennett,^c the subject is not adverted to.

As far as I have been able to make out, Dr. Hudson's cases previously mentioned were the first recorded examples of tympanitic resonance in pneumonia, *not due to the presence of air in the cavity of the pleura*, and that Cases II. and VII. in his category, which so closely resembled my second case in all essential particulars, were of this nature, I have no doubt whatever, although he would seem not to have equally satisfied himself upon this point.

Case III.—A tradesman, in easy circumstances and of exceedingly intemperate habits was attacked, about 20th September, with what seemed to be delirium tremens of a low and asthenic form. I saw him on the 27th, with Dr. Cahill of Dame-street, when his condition was the following:—Pulse 150; respiration 42; great dyspnea; face flushed; conjunctivæ somewhat jaundiced; tongue loaded; urine scanty, and tinged with bile. There were insatiable thirst and constant retching. There was tenderness on pressure over the stomach and liver, and the latter was somewhat enlarged. The right side of the chest was dull anteriorly from the clavicle to the mamma; the dulness extended backwards to the axilla, but the postero-lateral and posterior portion of the chest on that side was perfectly clear on percussion. Over the anterior portion, coextensively with the region of dulness, bronchial respiration and coarse crepitus were audible, whilst posteriorly respiration was normal. There was occasional cough with expectoration of viscid rusty-looking sputum. Right side to be dry-cupped; a large warm poultice to be subsequently applied, and then a blister. Sulphate of quinine, 2 gr.; watery extract of opium, $\frac{1}{2}$ gr.; three times a day. Six oz. brandy and beef-tea.

Sept. 28.—Pulse 168; diarrhea and bilious vomiting. Pleuritic

^a Cyclopædia of Pract. Medicine. Article, Pneumonia, p. 403.

^b Clinique Médicale de l'Hotel Dieu, de Paris. 1865.

^c Clinical Lectures on the Principles and Practice of Medicine. Fourth edition. 1865.

friction in postero-inferior portion of right side. Quinine, gr. 3; powder of ipecacuanha and opium 2 grains every fourth hour; $\frac{1}{2}$ oz. of mercurial ointment to be rubbed into axillæ. Bismuth liquor, 30 drops, tinct. opii, 5 drops, in an ounce of water when vomiting was troublesome. Four oz. beef-tea with $\frac{1}{2}$ oz. brandy every second hour.

Sept. 29.—Pulse 168; respirations 42; tongue moist and furred; has taken, without permission, a pint of brandy since yesterday; vomiting checked; diarrhea. Dulness on right side in front from clavicle to nipple, with bronchial respiration and coarse crepitus; lateral and posterior regions clear, with exception of upper portion of scapular region which is dull, and here bronchial respiration is heard; elsewhere posteriorly breathing is vesicular. Repeat large blister in front; to be followed by succession of warm poultices. Quinine and Dover's powder to be continued. Mercurial ointment, as before; beef-tea; brandy; arrow-root.

Sept. 30.—Pulse full, regular, 158; respiration 42; tongue moist and cleaning at the edge; was tortured with horrible dreams in the course of the night, which he requested to be relieved from; diarrhea of a dysenteric character; slight mercurial fetor. Dulness rather less extensive in front (from clavicle to one inch and a half above nipple); no respiratory sound of any kind in dull region; clearness behind and laterally.

October 1.—Died at 4, a.m., to-day.

This was an example of typhoid pneumonia engaging the superior lobe of the right lung, in a person who was the subject of alcoholic poisoning; and, as is usual in such cases, it ran a rapid and fatal course. At a time when there was evidence of improvement fatal syncope occurred. It has been introduced into this category mainly for the purpose of showing, in contrast with the case of Dempsey (No. I.), that solidification of the upper lobe of the lung, the inferior lobe being in a healthy condition, is not of necessity accompanied with resonance, as the doctrine of Skoda previously adverted to would imply. It also disposes of the hypothetic solution of Dr. Williams, who suspects that the examples of pneumonic resonance given by Graves, Stokes, and Hudson, "were examples of the production of tracheal or amphoric sound, *from consolidation of the upper lobe of the lung*, and not pneumothorax."

In the case just narrated the condition supposed by Dr. Williams to give rise to this phenomenon existed, and yet it was not developed.

As to the treatment pursued in these cases, it might be described as being of a mixed character, but rather stimulating and restorative than depletory.

In the second case given bleeding from the arm was practised, the only instance in which I have done it since I became an hospital physician, to relieve embarrassed respiration and prevent the patient from being suffocated, rather than with a curative object. The pulse was at the time full and strong, and blood was drawn only to the amount of seven ounces, with much relief to the breathing. At the same time and subsequently wine was given freely.

Neither this case nor that last given can be adduced as affording testimony in favour of the stimulant plan of treatment, as both cases were fatal. Yet a consideration of their gravity, including their antecedent history, will probably warrant the assumption that under no system of treatment would recovery have taken place in these cases.

In the *Journal of Practical Medicine and Surgery* for April, 1865, is given the substance of a paper by M. Béhier, extracted from the *Bulletin de Thérapeutique*, in which the author states that he has treated thirty-four cases of pneumonia with alcohol, with a result of twenty-seven recoveries. His method is to give from three to ten ounces of brandy in the course of the day, diluted with an equal quantity of water, in tablespoonful doses.

In my first case the evidence afforded in favour of alcoholic treatment was of the most striking and decided character, as will be seen by reference to the report. It was no less so in the case of a woman named Farrell, of which I have preserved only the following particulars:—

Eliza Farrell, 6, Greek-street, aged forty, admitted May 17. Had cough throughout last Winter, which she attributed to a cold and damp room. On the 13th of May had a rigor, accompanied by a sensation of general uneasiness, rather than pain, in front and back of chest. For these symptoms had got some medicine at one of the dispensaries, but having experienced no relief sought admittance into hospital.

When admitted patient was in a state of great prostration and respiratory distress; pulse 120 and weak; tongue dry and crispy; face pale, and surface cold; frequent hacking cough, with brown and viscid sputa.

Signs.—Well-pronounced dulness in right sub-clavicular and scapular regions, where likewise was heard bronchial respiration with a few crepitant râles. Other portions of right side of chest and entire of left side resonant, and respiration exaggerated, but in other respects normal.

R.—Sulph. quinæ, gr. xxxii.; ether sulph., 3 i.; acid sulph. dil. 3ss.; aquæ. ad 3 viii., st. coch., mag. 3tia. Quaque hora. Strong beef-tea *ad libitum*, and four ounces of whiskey daily.

Inflammation progressed downwards, and ultimately engaged the entire lung; but under the above treatment, which was persisted in, patient made a perfect recovery, and was discharged June 4, *i.e.*, eighteen days from the date of admission.

This was a good example of typhoid pneumonia, and as no other treatment save that by stimulants, nutrients, and quinine was resorted to, it illustrates the favourable results of the restorative plan of treatment advocated by Professor Hughes Bennett.

The conclusions deducible from these cases, as regards the phenomenon of muffled tympanitic resonance, are both negative and positive.

Negative.

1st. The phenomenon is not due to transmitted resonance from a healthy through a solidified portion of lung substance.

a. Because in Case No. II., in which it was best pronounced, the *entire* lung was solid.

b. Because in Case No. III. it did not exist, although the inferior and posterior portion of the lung was physically healthy.

2nd. It was not the result of gastric resonance transmitted through a solid lung.

a. Because it existed in Case No. I., in which only the superior portion of the left lung was hepatized, the inferior lobe being in a healthy condition.

b. Because in Case No. II. it existed only in the superior portion of the right lung, notwithstanding that the entire organ was solid; and it likewise existed in the *isolated* lung, as proved by *post mortem* test.

3rd. It was not due to pneumothorax.

a. Because in Case No. II. it existed up to death, and after death lung was found universally adherent to chest, and no air existed in the pleura.

Positive.

1st. The phenomenon was intrinsic in the lung, and had its seat in that portion of the organ in which it was manifested.

a. Because percussion of the lung removed from the body afforded proof of its existence in the isolated organ, and even in a thin layer of it resting on a solid body.

b. Because percussion showed dulness in that portion of the lung, after removal from the body, over which dulness existed during the patient's illness.

2nd. The resonance of the solidified lung was associated with the presence of air in its tissue.

a. Because air freely escaped from an opening made with the finger, under water, in that portion of the lung which yielded tympanitic resonance.

3rd. Simple pneumothorax, whether pneumonic or pleuritic, is characterized by *absence of respiratory sound*, coextensive with tympanitic resonance, and by displacement of the heart if the aëriform effusion be abundant, as in the cases of Graves and Little.

4th. The resonant or tympanitic dulness of pneumonia, due to air implicated in the tissue of the lung, is distinguished from pneumothorax by the qualified or muffled character of the resonance, and by the presence of bronchial respiration and of crepitus, as in Dr. Hudson's cases and in mine.

ART. IV.—*Contributions to Practical Surgery. On the Treatment of Granular Ophthalmia by Pressure.* By WILLIAM STOKES, Jun., M.D., Ch. M. Univ., Dub.; L.R.C.S.I.; Surgeon to the Meath Hospital and County of Dublin Infirmary; Lecturer on Surgery, Carmichael School of Medicine; Fellow of the Royal Medico-Chirurgical Society of London; Corresponding Member of the Hufeland Medico-Chirurgical Society of Berlin, &c.

I HAVE little doubt that, in this country at least, one of the most frequent causes of incurable blindness among the poorer classes, is that form of granular inflammation of the conjunctiva described by oculists under the various names of "granular lids," "conjunctivitis granulosa," "trachoma," &c. One cannot but be convinced of this on visiting some of the remote districts of this island; and it is truly melancholy to see what vast numbers of otherwise healthy

men and women, are to be found in the union workhouses, and the various blind asylums of the country, deprived of all means of self-support, in consequence of this distressing affection, or of some of its numerous sequelæ. Whether it is that the humbler classes of this country are not so well housed, clothed, or fed, as those of our wealthier neighbours at the other side of the Channel, or that there are some atmospheric or geological peculiarities which tend to induce the disease, it is impossible to say. But as far as my experience goes, the disease, as observed in Ireland, is of a more formidable type, and is more intractable to treatment, than in England, and many of the other European countries, excepting perhaps Bohemia. The surgeon engaged in practice in Ireland, especially, should give a careful consideration to any suggestion based on a sound surgical principle, for the improved treatment of this grave disease; and the method I wish to advocate is, I think, founded upon such. Let me not be understood, however, to advocate this treatment to the exclusion of other methods which surgeons have adopted for the cure of this disease. But I hope to be able to show that pressure—which has nothing in it antagonistic to other therapeutic measures which may be simultaneously adopted—applied in the manner I shall describe, should be allotted a high rank among the therapeutic agents which are made use of in the treatment of granular ophthalmia.

In this communication I shall not enter into the question as to whether this affection depends on any constitutional dyscrasia, or is merely the product of some local irritation. The anatomical nature of conjunctival granulations, which are not new formations or growths, but merely an abnormal or pathological condition of previously existing elements, and the facility with which, under certain injurious influences, the disease can be induced, point much rather to its being a local affection, than, as is generally held, a mere expression of a strumous diathesis. Professor Stellwag is also of this opinion, and observes that the injurious influences which induce trachoma are the same which induce catarrh, blenorrhea, pyorrhœa, herpes, &c.; and that among them impure air, dust, smoke, acrid vapours, exhalations, &c., are the most prominent. “Why these influences,” the Professor observes, “in one case produce one form, and in another case another form of conjunctival inflammation, is not explained. With regard to trachoma, we cannot be mistaken in assuming that the more frequent or uninterrupted influence of such pernicious agents, and the long-continued

maintenance of a certain condition of irritation in the conjunctiva, constitute a predisposing condition. It is natural, then, that large barracks, poorhouses, houses of refuge, educational institutions, lodging houses, large sleeping rooms for workmen—places, in fact, in which a large number of men live together, and where the cleanliness and ventilation is defective, serve as niduses for trachoma; while individuals who are subject to such influences only occasionally, after long intervals and for a relatively short period, suffer less frequently, and become affected instead with a catarrh or a blenorrea.”^a Such are the views of Professor Stellwag with regard to the etiological conditions which produce this affection. I shall, however, refrain from discussing at greater length this part of the subject, but proceed to consider what I believe to be the most rational method of treating this formidable affection.

Every practical surgeon must at once acknowledge the very great importance of pressure in surgical therapeutics. The beneficial effects of it in the treatment of urethral stricture, in many forms of chronic articular disease with thickening of the synovial membrane, also in the treatment of exuberant granulations on unhealthy ulcerated surfaces, are familiar to everyone of even limited hospital experience.^b

The physician, too, has occasional opportunities of observing the beneficial effects of pressure in arresting morbid local action. I allude here particularly to the interesting fact, as observed by my father, of the arrest of the constitutional symptoms of pulmonary consumption in certain cases of pleural fistulæ, and consequent empyema and pneumothorax. This circumstance has been observed

^a *Lehrbuch der practischen Augenheilkunde.* Von Dr. Karl Stellwag, von Carion, s. 391.

^b The treatment of anthrax by pressure, first suggested and practised by Mr. O’Ferrall, of St. Vincent’s Hospital, in this city, has engaged the attention of many distinguished surgeons, and amongst them are still found many warm advocates for it. From inquiries into the pathology of this affection, I am inclined to the opinion that the old system of crucial incisions, provided they are sufficiently deep and complete, appears to be the most rational mode of treatment. The distinguished French surgeon, M. Nélaton, who had abandoned the treatment by incisions, now recommends it as being the only one likely to be of real benefit. It is, however, by no means necessary that all cases of anthrax should be so treated, the free use of the bistoury being only advocated by M. Nélaton in the cases where the anthrax has an indurated base. Mr. Hugh Carmichael has advocated the use of pressure in certain cases of venereal phagedenic ulceration (*Dublin Journal of Medical Science*, September, 1838). Not having, however, had an opportunity of witnessing the effects of this practice, I am not in a position to verify or disprove Mr. Carmichael’s statements with regard to the efficacy of this procedure.

in several cases in the Meath Hospital, and seems to be attributable to the pressure exercised on the diseased and collapsed lung. Certain it is that as in several cases observed in Dublin, the patient, after having recovered from the shock produced by the accident, and the consequent effusions of air and liquid into the pleura, may experience a gradual improvement in general health, and ultimately so far recover that all constitutional symptoms of phthisis disappear for a length of time.^a

Cases, too, have been observed of the disappearance of splenic tumours under continual pressure. The late Mr. Kirby, of this city, used to detail a remarkable example of this. Atrophy of the lung, to the last degree, has been noticed by Andral, in his *Clinique Médicale*, as a result of pressure by a mediastinal tumour; and the termination of cases of aortic aneurism and intra-thoracic cancer, by sphacelus of the lung, is an example of the effects of pressure in modifying normal organizing processes.

There seems some reason to believe also that, even in essential diseases, the production of the secondary organic effects—as, for example, the development of pustules in small-pox, may be prevented by early modification of the local vascular action by pressure. The following case, already noticed by Dr. Stokes in his paper on the prevention of pitting in small-pox, is of such extreme clinical interest that I shall make no apology for quoting it:—

“A man, who had been under treatment in the surgical wards of the Meath Hospital for a chronic disease of the knee-joint, became affected with fever, followed by the appearance of variola of a severe type. He was transferred to the medical wards, and went through the stages of the disease in its confluent form. His knee had been strapped with mercurial plaster; and at the period of retrocession, and the falling of the scabs, the dressings came off. We then observed this singular appearance:—Above and below the knee, and ending and commencing at a line exactly corresponding to that of the strapping, the integuments were covered with blackened scabs, while the knee remained completely free of any sign of eruption, and silvery white, contrasting strangely with the condition of the parts above and below it.”

There can be little doubt that in this case the arrest of the

^a See article “Pneumothorax,” in the *Cyclopædia of Practical Medicine*, by the late Dr. James Houghton. Similar circumstances are detailed by Dr. Stokes, in his work on *Diseases of the Lungs and Windpipe*.

pustular development was due to the diminution or modification of the vascular action in the part by pressure, and I think it also probable that the treatment for the prevention of pitting of the face in confluent small-pox, which is adopted with such signal success in the Meath Hospital by the application of poultices, is, to a certain extent, due to the same cause. These observations tend strongly to show that the development of pustules may be, at all events, considerably arrested by diminishing vascular action. Does the converse of this hold good? Undoubtedly. The development of pustules is observed to be especially confluent in those places in which, previous to the pustular formation, a determination of blood has been induced. For example—in situations to which mustard poultices may have been applied to relieve pain previous to the development of the eruption.

The beneficial effects of pressure are, however, much more frequently observed in many forms of the so-called surgical diseases. "By diminishing," as Professor Stellwag observes, "the calibre of the vessels, and consequently hyperemia and exaggerated production of elements," it acts as a direct antiphlogistic; and it is, therefore, of paramount importance in dealing with many forms of ophthalmic disease, and in the after-treatment of ophthalmic operative procedures. Bearing these facts in mind, and those of the case I have just adduced, may we not reasonably suggest *the great probability of the pressure bandage serving as a powerful prophylactic in the ophthalmic complications of variola?*

Von Græfe has recently discussed the subject of pressure bandages in ophthalmic therapeutics (*Archiv. f. Ophthalm.* IX., 2 p. 111–152. 1864). He commences by dealing with the subject historically, and mentions that even in the early records of ophthalmic surgery we find the pressure bandage made use of in cases of exophthalmos, staphylomata, diseases of the lachrymal sac, injuries of the bulbus oculi, &c. Among modern ophthalmists Sichel and Arlt^a have the merit of directing particular attention to the importance of pressure bandages, especially in connexion with the after-treatment of extractions. In addition to the cases of flap extraction, in which the pressure bandage is so valuable, Von Græfe discusses the value of it in cases of linear extraction, intra-ocular hemorrhages, partial escape of the vitreous body after removal of staphylomata; injuries such as perforation of the sclerotic, with partial prolapse of the choroid; also to promote healing in the after-treatment of opera-

^a Jahrbuch der Gesammten Medicin, B. 123, P. 238.

tions engaging the conjunctiva or lids—in cases of purulent infiltration of the cornea, corneal ulcerations, with prolapse of the iris, and in pustular or ulcerative destruction of the cornea.

Although, as far as I can determine, no mention of the treatment of granular ophthalmia by pressure, in any form, has as yet appeared in the literature of ophthalmic surgery, it has, nevertheless, been applied. In some of the Egyptian hospitals, as I have recently learned from a trustworthy authority, a favourite treatment for granular lids, which so frequently occurs as a sequela of the so-called Egyptian ophthalmia, is to strap down the eyelids by layers of adhesive plaster. This must produce a certain amount of pressure, which, most probably, accounts for the treatment proving so beneficial. I have also learned from Dr. Wordsworth, of the Moorfields Ophthalmic Hospital, London, during a visit which he paid to Dublin, last Autumn, that he has treated some cases of granular ophthalmia solely by the application of compress bandages, and with satisfactory results.

It was during a conversation with my friend, Dr. Robert M'Donnell, on the subject of granular ophthalmia, that it occurred to me that pressure would, most probably, best succeed if applied to the granular surface of the conjunctiva palpebrarum, without, at the same time, causing any undue pressure on the bulbus oculi. Dr. M'Donnell had already, in some cases of granular ophthalmia, inserted between the affected lids and ball of the eye masks made of glass and of the form of the ordinary artificial eyes, for the twofold purpose of preserving, if possible, the cornea from the mechanical irritation produced by the granulations, and also to subject these to a certain amount of pressure. Owing, however, to the difference in the convexities of the mask and the bulbus oculi the amount of pressure on the latter produced at the edges of the mask prevented the patient from bearing the instrument beyond a short space of time. The problem to solve, therefore, was to devise some means by which a continued pressure could be applied to the granular surface of the conjunctiva, without, at the same time, distressing the patient by any undue pressure on the bulbus oculi. After a number of experiments, the instrument which I devised, as represented here, appeared to me, at first, to possess all the necessary requirements. The principle of the instrument will be at once understood by a glance at the accompanying woodcut.



It consists of two highly polished ivory plates made somewhat thicker at the upper part corresponding to the reflected portion of the conjunctiva, at which situation the granulations are generally most abundant and of largest size; at the lower extremity of the inner plate a very delicate spring, made of gold wire, was attached, terminating in a broad button, which made slight pressure on the anterior or convex surface of the ivory plate. The outer or anterior ivory plate is removable, there being no attachment to the broad button at the end of the gold spring. This instrument I found easy of application and capable of being borne by the patient without any difficulty. The inner plate being first introduced under the eyelid, and the anterior plate then being placed on the outer surface of the eyelid, was held in its place by the spring.

One disadvantage of this instrument was soon obvious, namely—

the want of power to regulate the amount or degree of pressure to be applied. To be able to regulate this is of great importance in treating the disease in different stages of its development. To rectify this deficiency Mr. W. Pearsall, a pupil of the Meath Hospital, and a skilful mechanic, devised and constructed a simple but ingenious modification of the first compressor I have described, and by which the pressure can be increased or diminished according as the surgeon thinks fit.



We may now proceed to consider briefly some cases of granular ophthalmia which have been treated by pressure in the Meath Hospital. The first of these was that of S. C., aged nineteen, who was admitted into the Meath Hospital on the 25th of last August. She had suffered from an acute conjunctival inflammation, three months previously; and, at the time of her admission into hospital, the conjunctival surface of both upper eyelids presented numerous and well marked examples of what are recognized by ophthalmists as pure papillary granulations. There was considerable impairment of vision in consequence of corneal opacity, depending partly on secondary keratitis, and partly on the well marked development of the superficial form of pannus, or pannus of the first degree, as described by von Græfe in his clinical lectures. In addition to these, there were several small ulcerations situated, for the most part, on the upper portions of the cornea.

I looked upon this case as one particularly well adapted for pressure, and the result showed that my anticipations were well founded.

I commenced by applying the lid-compressor for one hour daily; and at the end of ten days, finding that the patient bore the instrument without any annoyance, and that there was a marked improvement not only in the affected portion of the conjunctiva, but also in the cornea, the pannus having to a great extent disappeared, I increased the time of application to two hours daily. This I continued up to the end of the fourth week. At this time the cornea and affected portions of conjunctiva began to assume quite a normal appearance, and an obvious improvement in the patient's power of opening the lids was observed.

The patient now could bear the instrument for four hours daily; and at the end of the seventh week I looked upon the case as one which had been brought to a successful termination. I did not, however, dismiss the patient from the hospital, as I wished to satisfy myself, if possible, that this signal improvement would remain permanent. At the end of three months the patient returned home.

The next case to which I shall allude is that of J. B., a young woman aged twenty-four, who was admitted into the Meath Hospital on the 20th of last September. She had been under treatment in the country for some time previously, and the surgeon under whose care she had been, had repeatedly scarified the granulations, and followed up this treatment by the free application of caustics. This case, which at the time of the patient's admission into hospital presented, at and about the upper portion of the reflected conjunctiva, a large cluster of well-formed granulations, I treated by combining pressure with applications of a weak solution of nitrate of silver. At the end of three weeks the patient, at her own request, left the hospital. The improvement certainly was very great, and showed, even in a more marked manner than in the former case, how easily the instrument is borne.

The third and last case to which I shall now allude, is that of C. H., aged eighteen, who was admitted into the Meath Hospital on the 12th of last October. This case presented an example of the highest degree of development to which pure granular trachoma may come. The palpebral conjunctivæ of both lids were thickly studded with the vesicular or "sago grain bodies," which, in consequence of their form and semi-transparency, the German oculists have not inaptly compared to frog-spawn. The swelling of the conjunctiva, at its reflected portion, was especially remarkable. The cornea presented all the characteristics of an aggravated form of secondary ulcerative keratitis pannosa. At

the time at which this report is being made (Dec. 9) the patient exhibits in every respect a most marked improvement both as regards the conjunctiva and cornea; and no treatment has been resorted to in this case except the application of the lid-compressors from two to four hours daily. These cases, few though they be, are sufficient to show that the treatment by pressure, applied in the manner I have described, can be adopted without discomfort to the patient, and without causing any irritation to a cornea, which is the seat of ulceration and pannus. The general improvement, in both conjunctiva and cornea, appears to proceed with more rapidity than it does under the ordinary method of treatment.

One great advantage which I claim for this treatment over the ordinary methods of scarification, and subsequent application of various caustics, is, that there is nothing in pressure which can tend directly or indirectly to the production of that most unfortunate, but too frequent result, not so much of trachoma as of its usual treatment, namely, entropium, with trichiasis. I have little doubt that this condition is more frequently the result of the use of caustics and the scarifying knife than the affection for which these are so frequently, and in many instances so injudiciously, made use of. Certain it is, that nothing can tend more to the development of cicatrices in the conjunctiva, and consequent contraction of that membrane and inversion of the lid, than the injudicious use of those ordinary methods of treatment to which we have before alluded.

The treatment by pressure is also quite free from all danger of the occurrence of diphtheritic and other destructive forms of inflammation of the conjunctiva, which not unfrequently supervene after the treatment by inoculation.

With these brief observations on the treatment of granular ophthalmia I shall for the present content myself, and trust, at no distant period, to be able to lay before my professional brethren, the results of a larger experience in the treatment of granular lids by pressure.

ART. V.—*Cases of Injuries of the Larger Joints, with Remarks.*
By W. COLLES, one of the Surgeons to Steevens' Hospital.

CASE I.—A boy, aged about thirteen, came in contact with a reaping machine, while in motion, and received the following injuries in and about the ankle joint:—There were three transverse incised wounds, with intermediate oblique incisions partially connecting them. The first of these, about an inch in length, was situated on the dorsum of the foot; the second was somewhat larger, and higher up; and the third about the level of the ankle joint, and exactly representing what would be the anterior incision in Syme's operation—it extended from one malleolus to the other, dividing integuments, tendons, arteries, and bones, and opening the ankle joint, into which the fingers could be passed in all directions. The extremity of the fibula was deeply incised; two detached portions of bone from the lower end of the tibia were lying along with the lower flap; these were removed. On admission into Steevens' Hospital there was little or no hemorrhage; the shock was not as great as might have been expected. Notwithstanding that the foot was flexed to the utmost, I was unable to close this wound; I therefore drew the lips together as well as possible by means of sticking plaster, without using sutures, and maintained the flexed position by means of an iron splint, which was made in one piece, extending from behind the calf to the sole of the foot; this, along with long straps of plaster from the toes to the upper part of the leg, kept the part perfectly at rest. In three days' time suppuration had commenced; but I did not allow the dressings to be removed, for nearly a fortnight, contenting myself with merely correcting the fetor by chlorinated washes, &c.

When the plaster was removed I was gratified to find a broad belt of granulations occupying the site of the extensive wound. The patient was kept at rest for a month, when he was allowed to get out of bed, but not to put the foot on the ground; in another month he returned home with a useful foot.

In this case the prognosis at first was not very favourable, nor was the prospect of a rapid cure very encouraging; indeed, if we had followed the principles which are laid down for our guidance, we should have at once proceeded to consider the propriety of having recourse to either resection of the joint or amputation. I attribute much of the successful issue to the youth and good

constitution of the patient, and to the fact that I did not allow the limb to be disturbed, nor even the dressings to be removed, for a considerable period, although the discharge was profuse and the odour disagreeable. I thereby guarded against the probability of inflammation coming on from disturbing the position of the limb, and also against the admission of the atmospheric air.

CASE II.—Mr. S., aged about thirty, a dissipated man, was admitted into Steevens' Hospital, in a state of extreme intoxication, having met with an accident for which he could not account. We found a fracture of both bones of the left leg, close to the ankle: there was a small punctured wound on the outer aspect, which was bleeding freely, so much so that it was the occasion of my being sent for. Although only an hour after the accident, there was distinct emphysema of the leg as far as the knee. In a few days a black spot was observed on the outer malleolus; a slough, about the size of a crown piece, separated, exposing the joint and a portion of the fibula bare and very movable. After a few days healthy granulations sprung up, the piece of bone becoming firmer and more attached.

The case was progressing favourably, the wound being nearly healed, when a phagedenic form of ulceration seized it; it carried everything before it; nor did it cease until it had come within four inches of the knee joint. Various caustics and stimulating applications were tried—even change of air had no effect in arresting its progress; finally, I covered the whole surface with brown sugar, when the ulceration ceased. As soon as the ulceration made no further progress I amputated below the knee. The wound was healing well, when a large abscess formed on the outer side of the knee, another on the inner side; the discharge was so profuse that at one time we were considering the expediency of amputating above the knee; however, after a time the discharge began to decrease. Abscesses continued to form along the thigh, causing much debility. Finally, all healed up, and he left hospital, after twelve months' treatment, with a stiff knee.

On superficial observation we have here only an ordinary case of compound fracture of the bones of the leg, yet there were circumstances which rendered the case peculiar. The hemorrhage was excessive, yet we could not go search for the bleeding vessel; it was to be feared that pressure would have had the effect of driving the blood into the deeper tissues of the limb. The wound was very

small, and must have communicated with the joint; the presence of emphysema, occurring as it did immediately after the receipt of an injury, indicated some serious mischief. The patient's health was none of the best, owing to his drunken habits, yet we had very little constitutional disturbance when the slough separated and the joint opened; this latter consideration alone was sufficient to preclude the question of amputation.

When the integuments sloughed to such an extent there was no other treatment remaining but to amputate as soon as the phagedenic action had ceased. The application of brown sugar stopped it completely. I had been reading, a few days previously, of its having been used by American surgeons during the late war, when no other dressing could be obtained; they reported so favourably that I made trial of it in this case.

CASE III.—A man, thin, but healthy, aged about thirty-two, was caught in an engine by the right knee. There was a lacerated wound on the inner side of the patella, with considerable swelling about the joint, and, as far as could be ascertained, a fracture of the patella; some serous fluid could be pressed out of the wound; there was considerable ecchymosis of the thigh.

After some days a dark patch was observed on the outer side of the patella; here a slough formed, one inch and a half long by half an inch in breadth; this was raised considerably, and gave the evident sensation of fluid underneath. I divided the slough, and gave exit to a considerable quantity of sero-purulent fluid; as an abscess had formed in the thigh, I opened it also, and let out some healthy pus. There was considerable pain and starting of the limb, especially at night.

In about three weeks the discharge had diminished considerably; but, as the wound did not close, I introduced a probe, and felt a portion of bone bare and loose; on its removal it was ascertained to be a portion of the under surface of the patella partly covered with cartilage. The knee had become stiff and rigid, but I thought I could feel some motion in the joint; I put it across my knee and gave it a forcible wrench, when I felt and heard a tearing and laceration of the tissues in the joint. Of course this gave him great pain, but was not followed by any inflammation. He would not allow anything more to be done to the leg, so he left the hospital a few days after.

Taking everything in the case into consideration, we must admit

that in this case the joint was opened, yet the constitutional disturbance was not great at any time.

CASE IV.—A boy, while swimming, struck his knee against some hard body. He was brought to Steevens' Hospital with an incised wound a little above the outer angle of the patella, extending outwards for about two inches; it was treated in the usual way. All went on well for about a fortnight, when, in the centre of the granulations filling up the wound, a yellow spot appeared resembling a hernia of the synovial membrane; it increased in size, and became elevated; it ultimately broke, and gave exit to a quantity of serous fluid resembling synovia. Acute inflammation of the joint followed, as characterized by swelling, redness, and pain. I at once put him under the influence of mercury. An abscess formed of considerable size on the inner side of the joint, and extending down over the tibia; I did not interfere with it locally, but allowed it to burst; it emptied itself and closed. Other abscesses formed behind, and again in front of the femur; eventually the pain subsided. The joint is now stiff; the periosteum all round is thickened; the patient is gaining strength and appetite; he is most anxious to get up.

In this case the wound at first did not apparently reach the joint, yet it implicated it subsequently, without any evident cause, and by a process not easily explained. We had all the symptoms of acute inflammation of the joint, terminating in the formation of a large abscess; yet matters did not go to that extent that we find in similar cases. I attribute this very much to the rest, and beneficial effects of the mercury, which, whatever theoretical detractors may say, has a great influence in counteracting inflammatory action.

We may perceive that the reparatory process may go on in a joint, though it be filled with purulent matter, and that ankylosis may occur without the cartilage being expelled by the profuse suppuration, as we find in *disease* of the same joint. The process by which the joint came to be secondarily opened is curious, and not explicable by any surgical process that I know of. In this case, also, the necessity of an operation was imminent.

CASE V.—A boy, aged about fourteen, received a contusion on the inner side of the left knee, from the wheel of a cart. He was admitted to Steevens' Hospital. Gradually a portion of the integuments in the neighbourhood assumed a dark colour, a slough

formed the size of half-a-crown, the line of demarcation showed itself, and sank deeper. He had no bad constitutional symptom up to this time, viz., about a fortnight after the receipt of the injury, when suddenly, at night, he was seized with violent pain in the joint, accompanied by great constitutional disturbance; the knee became greatly swollen and red, the tumefaction extending up the inner side of the thigh, which soon assumed a reddish blush, gradually darkening in colour; on pressure at the knee a watery fluid, mixed with bubbles of air or gas, escaped; the fever continued to increase in intensity, he became delirious, and died in a few days.

On admission, this boy's case seemed deserving of little attention; yet when the separation of the dead from the living parts reached the joint, such local and constitutional disturbance was set up that the question of surgical interference was precluded altogether; in fact, to have given this patient a chance, the limb should have been removed on his admission, an operation which the surgeon would not have thought of proposing or the patient of submitting to.

In these injuries we can seldom expect a speedy and safe issue; we may perceive that there are different periods at which danger may be apprehended, and that they do not depend on the amount of injury. We may have reaction set in and inflammation run high in twenty-four or forty-eight hours after the receipt of the injury; this we may denominate the *primary period* of danger. At the end of the second or third week the separation of a slough, or some other unexpected cause, may give rise to a bad form of inflammation of the joint; we may denominate this as the *secondary period* of danger. These evil consequences may, I think, be attributed to the presence of air in the joint, and although many surgeons deny that this can set up inflammation, and even record cases where its presence has not been followed by any bad symptom, still when we consider that every part of the body is covered with cuticle, and that if we remove that covering, or expose a new surface, inflammation is set up, and a new covering is formed resembling the former cuticle, we may fairly reason that the presence of air in a joint is likely to be followed by inflammation. Even when the viscera are exposed a covering of cuticle is thrown over them, as in the case recorded by John Bell. Having escaped these periods of danger we have the exhausting discharge of pus in quantities, the ulceration and destruction of cartilage, the effusion of lymph, granulations, adhesion and consolidation into bony union, to contend with. During all this time the patient is liable to all

the various diseased actions attending all injuries and surgical operations. This we may denominate the *tertiary period* of danger.

From the cases here recorded we can scarcely draw any deductions on which to form rules for our guidance in practice; on the contrary they show us that the rules in vogue at present are liable to many and great exceptions; they compel us to go back and take up each case as a separate consideration, and to recommend a line of treatment which we think most suitable for that particular case. This includes the important question of operation.

In injuries of the larger joints the surgeon has often little difficulty in deciding on the line of practice to be adopted and the dangers to be dreaded. Many cases thus die of the constitutional effect of the injury a few hours after its receipt; we may have cases in which the surgeon can see at a glance that it is at once necessary to remove the part by resection or amputation. On the other hand, there are many cases which are sources of much anxiety, as to the consideration of which line of practice he should adopt. For the solution of this momentous question he must rely on his experience—both on what he has seen and read with the circumstances in the patient's case. If he determine to save the limb he must look forward to long suffering, great exhaustion, and at best a termination in a stiff joint, during which, at any time, an attack of inflammation may supervene and destroy the patient, without being able to have recourse to any operation; or if we must resort to operation it must be under circumstances much less favourable than at the outset. But if the operation be proposed at the first he may hope for recovery in a few weeks.

Having decided on the necessity of an operation, another question arises, what it will be—resection of the joint, or amputation? The records of civil practice do not offer us many cases from which to judge of the probable success of resection, but we must not forget that it in general is a much more severe tax on the constitution than amputation. In military practice, where the injury is so greatly complicated, though in smaller joints, resection may succeed; yet in the larger joints we find the operations, resection and amputation, all have such a tendency to a fatal termination that it is difficult to form any opinion from the statistics at our disposal.

One thing we learn, that the usual manner of evading the difficulty by proposing to wait, and be guided by symptoms as they arise, is not to be considered always as safe and expedient. If we defer operating, on the receipt of the injury, we may never again have

a chance of affording even such partial relief; the attack may be so sudden and severe as to preclude the possibility of an operation as occurred in the last case.

Here I may allude to the weakness of some surgeons who, in cases similar to these, say they will explain everything to the patient, and let him decide as to the expediency of an operation. This is an abandonment of the duties imposed on him, and an unworthy effort to ease his conscience by throwing the responsibility on the shoulders of a patient who is perfectly unacquainted with surgery, and at the time incapable of forming a correct judgment. The surgeon cannot always say decidedly whether or not the operation is absolutely necessary to save the life of the patient; for surgery is not perfect, nor surgeons infallible. He should, however, say, to the best of his judgment, whether or not the operation affords the best chance of recovery. Should he urge this with all the power he is capable of, he has done his duty; if the patient refuse, the surgeon must only adopt that other plan of treatment which to him appears next best; this he may pursue with an easy conscience.

ART. VI.—*On Scarlatina Complicating Child-bed.*^a By ALFRED H. M'CLINTOCK, M.D., F.R.C.S., President of the Dublin Obstetrical Society; Honorary Fellow of the Obstetrical Society of London; Late Master of the Lying-in Hospital, Dublin; &c., &c., &c.

IN the present paper I propose submitting a clinical analysis of some cases of scarlatina, occurring within the first eight days of childbed.

Common observation has attached a great deal of importance to the disease under these peculiar circumstances, and by physicians generally such a complication of the puerperal state is regarded with profound dread. Nevertheless, but little has been done towards investigating scarlatina under this aspect. Beyond a few isolated cases recorded here and there in our periodic medical literature, I believe we may in vain search for information upon the subject.

^a This paper was read at a meeting of the Medical Association (K.Q.C.P.), in February, 1859. It is now published, with little alteration, beyond the addition of some cases.

Although not just prepared to say that scarlatina, as met with in the recently-confined woman, assumes a character or runs a course necessarily different from what it does at other times, still, when we look at the condition in which the whole economy is left by the act of parturition—the exalted sensorial and nervous sensibility—the disturbed circulation—the facility for contamination of the blood—the proclivity to abdominal inflammation—the augmented activity of the absorbent and secreting functions—and the exhaustion of the patient; when, I say, all these facts are taken into consideration, there will appear good reason for supposing that the disease must undergo at the puerperal period some considerable modification in its symptoms and course.

Let me, in the first place, speak of the mortality resulting from the disease, when invading puerperal patients; and, be it remembered, it is to such cases exclusively that my remarks upon scarlatina throughout this paper have reference. Of all the cases I find recorded (their number is very limited), as well as of those furnished to me by medical friends, the fatalities were two out of three, or over sixty-six per cent. From this return the disease would appear a truly formidable one—in fact, not much less to be dreaded than puerperal fever itself. I am happy to be able to say that this fearful mortality does not always attend upon it. Of thirty-four cases of scarlatina occurring among the patients of the Lying-in Hospital, from November, 1854, to November, 1861, only ten died: that is, in other words, a death-rate of thirty per cent. There is, undoubtedly, a wide difference between these proportions; but if we take their mean, namely, forty-eight per cent., we shall probably have the nearest approximation to the average mortality from the complaint when affecting puerperal women. Even so, however, it appears a formidable disorder, and is justly to be viewed as one of the most dangerous complications of childbed; scarcely less so than epidemic puerperal fever, from which it seems to differ in this particular, that whereas the latter is more fatal in hospital practice, scarlatina, on the other hand, is, if anything, more fatal in private practice.

There is one circumstance which, beyond all question, exercises a very decided influence upon the fatality of the complaint, and that is the period of child-bed at which its invasion is made. As a general rule, *the earlier its invasion the greater is the danger to be apprehended.* Of the ten fatalities among the cases which constitute the basis of this memoir eight were patients in whom the disease

appeared within thirty-six hours after delivery. In the two other cases it seized the women on the third day. Altogether, there were *eighteen* patients attacked on the first or second day; and, as we have just seen, *eight* died; that is, in the proportion of about forty-five per cent.; whereas all those in whom the disease appeared on or after the third day—*sixteen* in number—recovered, with only two exceptions. These statistics would go far to establish the point in question; but they are strikingly confirmed by the observations of Dr. Halahan, reported to the Obstetrical Society. Of Dr. H.'s cases, *three*, who were ill from the moment of delivery, died. Of *five*, who were attacked during the first twenty-four hours after delivery, but *one* recovered. Of *ten*, who were attacked on the second day, but *one* recovered. Of *four*, seized with the disease on the third day, but *one* recovered; the remaining *three* were attacked with the disease on or after the fifth day, and in each instance recovered.

A similar observation has been made in respect to puerperal fever, and was very well exemplified at the Lying-in Hospital during the fatal epidemic of this disease, which prevailed to such an extent in this, as in many other cities, during the Winter of 1854–55. "Thus, of *nine* patients in whom the complaint manifested itself on the first day of childbed, *eight* died (*i.e.*, '88); *twelve* were affected on the second day, *six* of whom died (*i.e.*, '50); *ten* were attacked the third day, and of this number the disease proved fatal to *three*, being in the proportion of thirty per cent."^a

With respect to the causes of death among the fatal cases of scarlatina, I find that *six* patients sank apparently under the direct influence of the disease; *two* died of metro-phlebitis; and two of peritonitis coming on at an unusually late period of childbed. These two deaths took place, the one on the twelfth and the other on the thirteenth day of childbed. In both of them the peritonitic symptoms declared themselves contemporaneously with the first appearance of desquamation, and proceeded with such rapidity as, in the debilitated state of the system, to bring about a fatal issue within thirty-six hours. Of the eight other patients, the days of dissolution were, *three* on the fifth, *two* on the sixth, *one* on the eighth, ninth, and eleventh days respectively, counting from the day of delivery.

I cannot say there was anything peculiar as to the mode in which

^a See "An account of the Recent Epidemic of Puerperal Fever, as it appeared in the Dublin Lying-in Hospital," by the author, in the number of this Journal for May, 1855.

the disease made its invasion. The initiatory symptoms were rapidity of pulse, hot skin, headache, and the strawberry tongue, with redness of the fauces. With reference to the *pulse*, I would just wish to make an observation. In perfectly normal convalescence from parturition, it is, I consider, an exceptional occurrence for the pulse permanently to beat over eighty strokes in the minute; nay, I have seen not a few cases where its rate was sixty, fifty-six, forty-eight, and even so low as forty-two in the minute, without any assignable cause for it. Hence, a permanent acceleration of the pulse to ninety-six or upwards should always awaken the vigilance of the attendant, and lead to a full and careful investigation of the patient's condition. By strict attention to this point I have, in some cases, been led to suspect the presence of the disease at a very early period, and when its other symptoms were very imperfectly developed.

The eruption was on several occasions very slow in showing itself. Thus, in one case, it did not appear for ninety-six hours, and in other cases not until fifty-six, forty-eight, and forty-four hours, without this delay seeming to have any unfavourable influence on the course of the disorder, as all these women recovered. In one instance, however, where an interval of forty-seven hours elapsed before the rash came out, the woman died, but not until the thirteenth day, and then of peritonitis. No abatement of the fever took place in any case consequent upon the appearance of the eruption. This last did not present anything peculiar in its character. It was generally most intense, and of the deepest hue, in the bad cases. In one instance the rash made its appearance in the course of the labour. It was this patient's first pregnancy, and her age was thirty-one. The first stage of labour was rather prolonged in consequence of a rigid condition of the os uteri. It was remarked at this period that her face was more flushed, and the pulse more accelerated than could well be accounted for by the circumstances of her labour. In order to promote the dilatation of the soft parts, I ordered her a warm hip bath,^a which had a twofold good effect;

^a Let me here say a word respecting the use of the warm *hip* bath in the treatment of rigidity of the os uteri. Ten years ago I brought this remedy into use at the Lying-in Hospital, and, from a very large experience of its use there, as well as in private practice, I can, with the utmost confidence, recommend it on the grounds of convenience, safety, and efficacy in the class of cases specified. It is entirely free from the objections which apply to the *general* warm bath, under the like circumstances, whilst, I believe, it possesses all the advantages to be derived from the use of the latter. I have never seen hemorrhage induced by it; but, of course it should not be employed if

for when I next saw her (in the course of two or three hours) I found that the head had cleared the os uteri, and that the scarlatinal eruption was well out on her face, neck, and chest. Her pulse was now 120. The head of the child being sufficiently low in the pelvis to admit of the easy application of the forceps, I deemed it prudent to employ it in order to terminate the labour. This course was adopted solely in consequence of the appearance of the scarlatina, and not from the presence of any urgent symptom connected with the process of parturition. The next day the rash was well out, and of a deep red colour: pulse one hundred and twenty: some swelling and considerable redness of fauces: tongue dry and brown. The symptoms assumed more and more of a typhoid character; and she died on the sixth day, the eruption still occupying the skin, with undiminished intensity. No ulceration of the throat took place, neither had she at any time vomiting or diarrhea, although there was some tympanitis of the abdomen. From the second day she was generally in a stupid, drowsy state, bordering on, but not actually amounting to, coma. This is the only case that has come under my observation where scarlatina unequivocally declared itself before the completion of labour. The vital powers at once gave way under the disease; and the liberal employment of diffusible stimulants, quinine, and nourishment of every kind, seemed ineffectual to avert or delay the fatal issue.

The affection of the throat was comparatively slight in all the cases; and in but two instances did ulceration, and that of a trivial kind only, take place. The exemption from angina forms, I think, a remarkable feature of scarlatina when occurring among puerperal patients. Such a complete immunity is hardly ever met with, I believe, among non-puerperal patients of adult age.

Where the disease ran a favourable course its presence did not seem to interfere in any way with the healthy discharge of the functions peculiar to childbed. The secretion of the milk and of the lochia, the involution of the uterus, the contraction of the vagina, &c., all went on undisturbed by the existing complication. Nor

any tendency to hemorrhage existed. There is no house in which it cannot be improvised. Patients generally derive great comfort from it, sitting in it for twenty, thirty, or even forty minutes, the temperature (100 Fah.) of the bath being maintained by additions of hot water. I have frequently repeated the bath with manifest advantage. To protect the patient from contact with the cold sides of the bath, above the water's edge, a dry sheet may be thrown over the entire bath, and she is then allowed to sit down. The sheet does not prevent the contact of the water with her person, whilst it does prevent the contact of the cold sides of the bath above the level of the water.

can I say that the access of scarlatina gave any increased tendency to the occurrence of puerperal fever; indeed the opposite opinion would, perhaps, be more consonant with facts. It is of importance to bear in mind that the greater number of these cases occurred at times when puerperal fever was more or less prevalent; and yet of the thirty-four cases only *one* woman died with uterine phlebitis, one with metritis, and two with abdominal inflammation, which latter may with as much reason be pronounced scarlatinal peritonitis as puerperal peritonitis, for it appeared at a much later period after delivery than does puerperal peritonitis; but at that particular stage of scarlatina, namely, on the subsidence of the eruption, in which, according to Copland, inflammation of the abdominal serous membrane is most likely to show itself. "Peritonitis," writes this author, "may appear as a complication of scarlatina, either consecutively of diarrhea and vomiting or independently of these. I have, however, rarely met with it (he continues) during the stages of eruption, but more frequently as a sequela of the malady, and in connexion with obstruction of the kidneys and anasarca." It may, then, fairly be questioned whether these cases were examples of puerperal peritonitis.

Among the patients that recovered were two who showed some symptoms of hysteritis about the time of the coming out of the rash; but these symptoms (uterine tenderness and slight pain) gave way under mild measures after the eruption became fully developed. In three or four cases I remarked some increase in the size and the sensibility of the uterus cotemporaneously with the appearance of the eruption.

No example of dropsical effusion supervening on the attack of scarlatina came under my notice. This observation should, however, be qualified by stating that the patients left the hospital before, or about, the time that this sequela is wont to show itself, so that I cannot say whether they all escaped dropsy, or what proportion of them had it; certainly no example of such came to my knowledge.

In several instances a very distinct re-accession of febrile symptoms appeared at the time of desquamation, commencing about the eighth or tenth day. In one case, which terminated fatally, this circumstance was strikingly exhibited. A delicate-looking woman was confined of her second child after an easy labour. The day following she seemed feverish; pulse 96; her eyes were heavy, and she had some cough; no uterine pain or tenderness, however.

On the third day she complained of headache; pulse was 100; she was thirsty, and the tongue was rather dry. She had small doses of blue pill and James's powder (Newbery's) at short intervals. The next morning these symptoms continued; she looked languid and oppressed, and had slept badly. On the chest and back were numerous small patches of a dull pink-coloured efflorescence, but whether belonging to measles, scarlatina, or typhus could hardly at this period be determined from the character of the eruption alone.

On the fifth and sixth days the rash came out more abundantly, and was of a red colour; on the trunk it was continuous, but on the limbs rather punctated, and rough to the touch; no soreness of throat; fever more decided; tongue dry; pulse 100, and weak. She has been getting diaphoretic mixture, wine, and beef-tea.

During the two following days there was no alteration in her condition; the pulse ranged at about 104. She continued to get wine and beef-tea, &c. The state of the abdomen had been daily examined, and found most satisfactory, being soft and entirely free from pain or tenderness in any part. The secretion of milk has been very scanty, but the lochial discharge has gone on without interruption.

The morning of the ninth day showed a considerable amendment in her symptoms; the pulse had fallen to 88; the tongue cleaning, and the rash fading off; but she complained of languor and prostration: was allowed egg flip made with brandy, beef-tea, small doses of carbonate of ammonia, and an anodyne draught at bed time.

Tenth day.—Her improvement progresses steadily; she is cheerful, and feels stronger; pulse 88; tongue clean and moist; ate an egg at her breakfast. I stopped the carbonate of ammonia as the bowels are a little relaxed, but continued the nourishment. On the afternoon of the succeeding day she had a marked change for the worse, the pulse rising to 110, and the tongue becoming dry and glazed, at the same time that she complained of weakness and despondency. Desquamation has just begun to take place. Bad symptoms appeared in quick succession, and speedily acquired an extreme degree of intensity; rapid pulse, sunk countenance, prostration, total disinclination to food, swelling, pain and tenderness of the belly. I need not particularize the symptoms or the details of treatment; suffice it to state that she died in forty-eight hours from the outburst of these fatal symptoms, and that a *post mortem* examination revealed intense peritonitis, with serous effusion.

Now, in this instance it will be remarked that the sudden and

unexpected reaccession of fever and the occurrence of peritonitis were exactly coincident with the commencement of desquamation. The case was a remarkable one in other points, but those I need not here advert to.

Respecting the *treatment* of scarlatina when affecting puerperal patients my remarks may be few and brief. I shall merely give the inferences which may be drawn from my past experience, without wishing to constitute them into rules of practice, or to extend them in any way. The disease in question is one that presents varying types, and therefore must require corresponding alterations in its treatment. In this, as in puerperal fever and other epidemic diseases, we are too much in the habit, I fear, of prescribing according to some fixed rules, some Procrustean plan of treatment, instead of exercising a free unfettered judgment upon each case, and looking at it *individually* and not *generically*.

Careful observation has strongly impressed upon my mind two precepts in respect to the treatment of scarlatina when attacking puerperal women.

One of these is, extreme reserve in the use of purgatives during the course of the disease, and especially at its commencement. I am well aware that this maxim applies to scarlatina whensoever occurring; but I believe it to be of special importance under the peculiar circumstances we are now considering. Experience shows (as Copland remarks) that any undue action of the bowels in the progress of scarlatina is apt to be followed by peritoneal inflammation; and this tendency must be greatly augmented in puerperal patients who, as such, are always more or less predisposed to abdominal inflammation. I may mention two cases which will serve to illustrate these remarks:—

M. M., aged twenty-eight, was confined, at 2, p.m., of her third child, after an easy labour. The next morning her pulse was 120, face flushed, and skin very hot. In the evening she complained of headache and soreness of throat; the rash was just beginning to show itself on the chest. She passed an unquiet night, and at early visit of third day I found the pulse 128, urgent thirst, tongue coated; bowels two or three times freely opened by a dose of the ordinary black bottle (senna and salts), administered this morning without my knowledge. The rash has come out a little more on the trunk. Towards evening she became much prostrated; the pulse rose to 132, and she was oppressed and drowsy. Wine was

now freely allowed her; nevertheless her condition for the next few days was such that I almost despaired of her recovery. There was great prostration, with recurring attacks of purging; and it was only by the liberal use of wine and brandy that her strength was kept up until the disease began to pass away. The purgative here employed was by no means drastic, nor was its operation violent, and yet its injurious effects were very manifest.

If the bowels very much need to be relieved it will be best accomplished in these cases by a lavement, or, at most, by a small dose of castor oil, or a mild rhubarb draught. In the following case even a dose of the former, given on the fourth day of the disease, was productive, I believe, of irreparable mischief.

M. B., aged thirty, was confined of her third child at 4 a.m.; her labour having been of only five hours duration. The next day she was hot and feverish, but there was nothing to account for these symptoms. Late in the evening, however, the characteristic rash of scarlatina began to appear on the chest, and the following morning was well out on the trunk. She was weak and oppressed, and the pulse 120.

Early on the fourth day she got half an ounce of castor oil with one drachm of compound tincture of senna. This operated rather briskly, late in afternoon. From this forward there was a constant tendency to relaxation of the bowels, so that her diet and medicine had to be regulated with a view to restraining their action. She died on the twelfth day, exhibiting unequivocal symptoms of peritonitis with effusion; for some days previously the belly was tumid.

I regret much to say that this patient got no wine or brandy throughout the progress of the disease. Her case occurred in the year 1854, shortly after my taking charge of the hospital, and I was not then aware of the very great importance of having early and free recourse to the administration of stimulants in treating the scarlatina of child-bed. In fact, my mind was then strongly imbued with the dread of giving a diffusible stimulant to a recently delivered woman; I confess it was some time before I could divest myself of this feeling, and venture to regulate my practice by a rational view of the patient's symptoms and actual condition.

This brings me to speak of the second point, in connexion with

the treatment of these cases. My own experience leads me to attach much importance to an early exhibition of stimulants; in fact, to commence their use as soon as the disease has appeared, unless some special contra-indication be present.^a It has already been stated that of the thirty-four cases contained in this report, six died of what we may call the primary or direct effects of the scarlatinal poison acting upon the nervous centres.

It is worthy of note that a considerable proportion of the cases in this clinical report were primiparæ, viz., twenty-one out of thirty-four; but the circumstances of age will perhaps sufficiently account for this, whether it be true or not that the liability to scarlatina lessens with advancing years after the period of puberty. The death rate is not very widely different in the primiparous and pluriparous classes; in the former six out of twenty-four having died, and in the latter four out of fourteen.

The frequency of scarlatina among puerperal women seems to vary much at different times; and I am inclined to think that the prevalence of the disease among this class of patients does not altogether depend upon the general prevalence of scarlatina, but that some other influence is in operation. Facts are as yet wanting to determine this point. Most of the above cases occurred in the years 1854 and 1855, and the remainder in the year 1861. Many of them were distinctly traceable to contagion before admission into hospital.

In Dr. Collins' Report of the lying-in hospital no mention is made of any such complication of childbed having presented itself, although his report extends over a period of seven years, viz., from November, 1826, to November, 1833.

Dr. Hardy's and my Report of the same hospital, embracing the years 1842-3-4, contains one case of scarlatina, which proved fatal on the seventh day. Now this was, I think, the only case of scarlatina, certainly the only fatal case, which took place in the hospital, from November, 1840, to November, 1847, which was the period of Dr. Johnson's mastership. For the first three years, at all events, of this septennial period, scarlatina was very prevalent in this city, as we know from the writings of Dr. H. Kennedy and of Dr. Graves.

In the Report of the hospital under Dr. Shekleton's mastership

^a In a paper read before the Obstetrical Society, Session 1862-3 (and published in No. lxxi. of this Journal), Dr. H. Halahan has corroborated most strongly the above remarks on the value of stimulants in the treatment of such cases as the above.

(from 1847 to 1854), published by Drs. George Johnston and E. B. Sinclair, only two cases of scarlatina are mentioned. I subjoin the the history of each of these cases :—

“One woman fell a victim to this disease (scarlatina) after a purely natural labour. She was admitted in 1854, aged twenty-five, for her first pregnancy, and delivered of a healthy girl, after a labour lasting but three hours. She was attacked with the disease on the day after delivery, and died on the twelfth day of the secondary fever.”

The other case occurred amongst those of forceps delivery. “Delay was in the second stage from inertia, and was of fifteen hours’ duration. During the latter half of this period the head made but trivial progress. Delivery was easily effected with the forceps, and a healthy girl extracted. On the evening of the second day she complained of headache and sore throat, and had a quick pulse. An anodyne diaphoretic was ordered. Next morning the rash of scarlatina appeared, and the case terminated fatally on the fifth day; the immediate cause of death having been rapid effusion into the ventricles of the brain.”

A very severe epidemic of malignant scarlatina appeared among the patients of the Vienna Lying-in Hospital in the year 1799, an account of which was published by Malfatti, in *Hufeland’s Journal*. This was a dangerous form of disease, proving fatal in most instances upon the third or fourth day. It presented a very low asthenic type, and was most successfully treated with stimulants; but no stronger stimulants than camphor, assafetida, castor, and such like, seem to have been employed. In cases where it seized the patient soon after delivery, its fatality was greater than when its invasion was deferred to a later period.

Before closing I would wish to observe, that some writers have applied to scarlatina, occurring in the puerperal state, the designation of “puerperal scarlatina,” which is a manifest error of nomenclature. Scarlatina is not a disease belonging to, or in any way dependent upon, the puerperal state; it is merely an intercurrent disease, accidentally complicating childbed, and therefore no more deserves to be called “puerperal scarlatina” than it should be called “traumatic scarlatina” if invading a patient confined to bed with a scalp wound; or than hooping-cough making its appearance in childbed should be styled “puerperal pertussis.”

ART. VII.—*On Chromidrosis*.—Thesis for Degree of Doctor Medicine, Trin. Coll., Dub. Read before the College of Physicians. By ARTHUR WYNNE FOOT, M.D.

THE term chromidrosis (χρῶμα, pigmentum ; ἰδρώς, sudor) is applicable to those conditions of the perspiration in which it assumes a colour—red, yellow, green, blue, or black. There are reasons for believing that all the pathological pigments which appear in this secretion owe their tints to modifications of the colouring matter of the blood, which colouring matter is being continually set free by the disintegration of the blood corpuscles; and being itself of a very indestructible nature, but at the same time capable of undergoing many chromatic alterations, it may, under peculiar circumstances, appear on the surface of the body, and impart very various tints to the cutaneous exhalations. The term chromidrosis is strictly to be confined to the transudation of colouring matter alone, without any of the cellular constituents of the blood, for, from the nature of the capillary wall, which is absolutely homogeneous and continuous as a film of collodion, it is not possible that particles so large as the blood globules could pass through without a lesion of the vascular membrane; when a lesion does occur from internal congestion or external violence the blood will appear on the surface of the body as a hemorrhage or as an extravasation, but not as a secretion. Therefore to qualify any coloured secretion appearing on the surface of the body for the title of chromidrosis it is necessary that microscopic examination should detect no constituent which would involve a rupture, or compromise the integrity of the capillary wall. The presence of crystals of urea, ("On Urea on the Skin and Uremia," translated by William D. Moore, M.D., *Med. Press*, May 3, 1865, p. 419) of chloride of sodium (Prout, in *Lond. Med. Gaz.*, Vol. XV., October 1834), or of urate of soda on the skin presents no difficulty on account of their solubility previous to excretion.

The integrity of the capillary wall must be particularly insisted upon in considering the red form of chromidrosis, and the test of that integrity will be the presence or absence of the red corpuscles; this test excludes many of the cases of the so-called *sudor cruentus*, all the cutaneous hemorrhages which supplement menstruation, and those which are due to the hemorrhagic diathesis and to purpura; it will also be found a useful help in detecting deception.

The red sweating then will become a more rare phenomenon than might have at first appeared, and the only trustworthy cases of it which I have been able to find are those of Landerer, an Athenian chemist (Buchner's *Report*, Second Ser., Vol. V., p. 234), and Heller, of Vienna (Thudichum *On the Urine*, p. 321). They observed it in the axillæ of fever patients; the condition of the blood in fever, it will be remembered, is one most favourable to the transudation of the colouring matter; under the influence of fever poison the blood cells, possessing little durability at any time, are peculiarly prone to disintegration, the blood is found unnaturally fluid, the red corpuscles loosely aggregated, in amorphous heaps, crenate, and mis-shapen, as if undergoing solution (Murchison, *Treatise on Continued Fevers*, p. 238). Landerer considered the red sweat to derive its colour from the urine pigment, which is so closely related to hematin that Parkes (*On Urine*, p. 30) thinks it is not going too far to assume the amount of urine pigment to be a measure of the metamorphosis of the blood cells; it appears in the fiery red pigment of febrile urine, and its excess in the renal secretion seems to indicate rapid alteration of hematin.

There can be no doubt that blood appears most unexpectedly on the apparently unbroken surface of the body, and it will be useful to contrast two well-marked cases of cutaneous hemorrhage with the coloured sweatings, to show that they have no analogy to the physical process by which perspiration passes to the outside of the body. In 1863 Franque, under the title of hemorrhagic sweat, published the case of a female, aged forty-five, whose catamenia were regular, but who had three times bled from the skin after long continued and severe convulsions, brought on by mental emotion. On a fourth occasion Franque himself was present; the patient had suffered for four days previously most violent pricking pains along the vertebral column, in the left ear, the forehead, the left arm; after the convulsions had lasted an hour, there broke out all over the body a copious sweat, which appeared red at the parts which had been the seat of pain. It derived its colour from actual blood, containing red globules. The convulsions then gradually subsided. As the skin appeared unbroken Franque presumed that the hemorrhage took place from the sudoriparous glands (*Syd. Soc. Year Book*, 1864, p. 179). In 1857 Huss, of Stockholm (*Archiv. Gen. de Méd.*, 1857, p. 165) published a case of spontaneous hemorrhage from the skin, without excoriation of the epidermis, which came

under his notice in the Seraphim Hospital. The seats of the hemorrhage were nearly all hairy—the scalp, the axilla, the pubis, the edges of the eyelids—and were all on one side of the body except the right ear; the exuded blood was of arterial colour, and had all the physical and microscopical characters of healthy blood. The exudation occurred at various intervals of time, and lasted for various lengths of time; it was generally provoked by mental emotion, but appeared independently of that cause; it had no connexion with menstruation, and paralysis of motion and sensation in both extremities of the left side followed the attack. The explanation offered by Prof. Retzuis was a temporary spasm on the venous side of the capillaries, the course of the blood being thereby partially intercepted; the pressure of the blood coming from the heart and arteries against the points where the circulation was intercepted occasioned a lesion in the capillary vessels or the finest twigs of the arteries, and the hemorrhage ensued. Vicarious menstruation was not concerned in the production of the phenomena in these cases, because in both the uterine functions were regular; and observations of Cruveilhier (*Anat. Path.*, Vol. IV., p. 201) show that cutaneous hemorrhages may exist with normal or even with profuse menstruation.

In 1842 Dr. Todd (*Dub. Med. Jour.*, Vol. XXII., p. 12) examined the opinion that hemorrhage might occur by a sweating of the blood through the coats of the vessels, without rupture; and the results of his examination were, that from microscopic investigation of the diameter of the blood cells, he was led to deny the possibility of the escape of blood, in an unaltered state, through the walls of the capillaries, and to assert that all hemorrhages take place by rupture of blood vessels. On this subject Virchow (*Cell. Path.*, p. 113) has more recently spoken in terms of the most positive certainty, and confirms the opinion of Dr. Todd, that a transudation or diapedesis of the blood through its limiting membranes, without rupture, cannot for an instant be admitted; and although we may not, in every individual case, be able to point out the exact site of the lesion, he believes it so inconceivable that the blood, with its corpuscles, should be able to pass through the walls in any other way than through a hole in them, that he declares the subject to be one on which discussion is inadmissible.

In cases of jaundice yellow perspiration has been observed. From the researches of Valentiner and Kerner the colouring matter of the bile seems to be identical with that of the blood.

The colouration of the secretion of the skin in jaundice is not to be received as an evidence of the most intense impregnation of the blood with bile-pigment, because Cheyne (*Dub. Hosp. Reports*, Vol. III., p. 269) observed this phenomenon when the indisposition was so slight, that until his patient's linen was found to have acquired a saffron colour there had been no intention of seeking medical advice. Chomel (*Académie des Sciences*, 1737, p. 69) was acquainted with this symptom, and Andral (*Clinique Médicale*, Tom. II., p. 373) describes a case in which the sweat coloured the linen yellow, and the urine contained the colouring matter of bile, although the skin and conjunctivæ exhibited no jaundiced tint. On the other hand, it is remarkable that when every other secretion of the body has a bilious tint, and the solid tissues are deeply stained, the perspiration will remain uncoloured. The colouring matter in cases of yellow perspiration due to icterus, is pretty well ascertained to be caused by cholepyrrhin, the pigment of the bile. Breschet (*Jour. de Magendie*, Tom. I., Paris, 1821) was the first to adduce arguments in support of the derivation of this pigment from the blood. Virchow's investigations upon pathological pigments have proved that under certain circumstances a yellow substance is formed from hematine, which in its relation to solvents and reagents, bears a close resemblance to cholepyrrhin; and the researches of Funke would appear to indicate the possibility of a direct transformation of hematine into cholepyrrhin. These hypotheses may throw light upon the pigmentary changes which are observed in pyemia, chloroform-narcosis, and after the bites of some venomous serpents, if it be admitted that there may be a conversion into yellow pigment of the hematine liberated from the dissolving cells of the liquified blood. In the removal of yellow pigment from the blood by perspiration, the relation between the duties of the skin and renal organs is well exemplified. Next to the kidneys, the sudoriparous glands take the most active part in its elimination, although the quantity of colouring matter which they excrete is always very inconsiderable in comparison with that thrown by the kidneys. The bile-pigment makes its appearance in the renal secretion sooner than it does upon the skin, and serous effusions in the various cavities of the body become tinged even earlier than the urine. The sudoriparous glands do not undergo any important changes of structure in yellow sweating—their contents appear of a yellowish tinge, but are not the seat of much pigmental deposition (Frerichs' *Diseases of the Liver*, Vol. I., p. 103). The cutaneous secretion

has been observed of a yellow colour during the exhibition of rhubarb. The brown and red resins contained in this drug have been shown by Schlösslerger to be the principles in it which communicate to the urine and perspiration a yellow or red colour, according to the alkaline or acid states of these secretions. Yellow perspiration has also been noticed after the long-continued use of saffron, from the absorption of the colouring matter of the stigmata of that plant. I have not been able to meet with any observation on the effect of santonine upon the perspiration; but this white crystalline substance, which is rendered yellow by sunlight, and is transformed by the alkalies of the blood into coloured modifications, sometimes gives rise to xanthopsy, a phenomenon also observed in jaundice.

The bile contains two colourless resinous acids, the cholic and choleic acids of Strecker, corresponding to the glycocholic and taurocholic of Lehmann, which can be converted in the laboratory, by the action of acids, into a colour-producing substance called chromogen by Frerichs (*op. cit.*, p. 91). The same colour-producing substance is formed in the living body after the injection into the blood of the colourless biliary acids. The chromogen formed experimentally outside the body will, by simple exposure to the air, or more rapidly by the addition of NO_5 , exhibit alterations of tints corresponding in every respect with the colouring matter of the bile, cholepyrrhin, a substance rich in carbon. In the cases where chromogen is formed after the injection of colourless bile into the living body the oxygen derived from respiration is referred to for the development of colour. It is known that none of the elements of the bile, except the colouring matter, can be found in the body even in cases of intense and universal jaundice. This conversion of the colourless biliary acids into a colour-producing substance seems analogous to what is observed after the administration of santonine, and may find a parallel in the conversion of white indigo into blue indigo. Several years ago Heller, of Vienna (*Archiv. für Path. Chimie*, 1853, p. 121), announced that there exists in many urines, both normal and morbid, a substance, itself colourless or nearly so, which gives rise by internal changes to several pigments; the urines in which it occurs are often pale, and poor in normal pigment, but generally very acid. He called this colourless substance uroxanthine. From the difficulty of isolation the exact nature of this substance was not understood till recently. Dr. Schunck, of Manchester, in examining some urines, observed in them a substance

identical with a body also discovered by him in his investigations upon indigo, and which he had named vegetable indican. There appears great reason to believe that this substance, which he called vegetable indican, is the same as Heller's uroxanthine (Parkes *On Urine*, p. 196). This indican, when treated with acids, yields six insoluble pigments, of which indigo-blue and indigo-red are the chief; and some soluble substances, such as sugar and leucine, which it will be recollected are also found in the liver. Heller's uroxanthine also, when acted on by acids, and even by simple exposure to the air, very easily yields a blue colouring matter, which he called uroglaucin, and which appears as similar to indigo-blue as the colourless uroxanthine was to white indigo. These statements point to a strong analogy between animal starch and vegetable starch, and between animal sugar and vegetable sugar. The connexion between hematine and the ordinary urine pigment is generally accepted; the connexion between hematine and biliary pigment is very probable; one of the effects of acids upon a resinous acid of the bile from which its pigment is obtained, is similar to what is observed by the oxidation of uroxanthine. Glycocholic acid, when acted on by SO_3 , has the property, upon exposure to the air, of forming gorgeously-coloured combinations. If the amorphous colourless mass resulting from the action of sulphuric acid upon this white resinous biliary acid, is placed upon a piece of filtering paper, it dissolves, and there is produced a ruby-red spot, which soon presents a blue margin, and after a short time assumes an indigo-blue colour (Frerichs, *op. cit.*, p. 90, note).

Leaving now the red and yellow perspirations, the way is partially cleared for the consideration of the next form of chromidrosis, or coloured sweating—that in which the cutaneous secretion is tinged blue. It will be impossible to avoid taking account of the occurrence of blue pigment in other secretions, such as the urine, the milk, and in suppuration. Dr. Purdon, of Belfast (*Dub. Quart. Jour.*, Vol. II., p. 445), has published the case of a woman, aged forty, who, suffering much from cardiac anasarca, became subject to a clear serous discharge from the trunk and limbs, so abundant as literally to wet her bed; this serous discharge occasionally ceased, and at such times the œdema and dyspnea were very much aggravated; after some time the discharge changed very much in its character, being nearly alternately blue and straw-coloured, or yellow, almost like pure bile. She was made aware of the advent of the blue discharge by a mouldy smell and prickly sensation

which invariably preceded it for twelve hours; the yellow discharge was not attended by either of these premonitory phenomena. The blue discharge always appeared along the posterior part of the chest, the yellow generally proceeded from the abdomen and back of the neck, and rarely from the back; the blue never appeared on the abdomen; the two colours were sometimes procured from the different parts at the same time; the discharge from the extremities was never coloured. The yellow colour was tolerably permanent, the blue faded; she had not taken any preparation of iodine for some years; in place of the catamenia there was a discharge of a reddish-green colour. Dr. Gibb (*Brit. Am. Jour. Science*, N.S., Vol. VI., p. 201, 1850-1) has collected ten cases of blue suppuration, including two observed in Dublin; in one of these cases, which was communicated to the College of Physicians by Dr. Croker (*Dub. Med. Jour.*, Vol. II., p. 367) the dark blue stain imparted to lint was examined by Dr. Apjohn, who pronounced that it was *not* Prussian blue, as had been previously believed. The subject from whom this coloured pus was obtained suffered from a morbidly increased secretion of gastric acid, so much so that her throat, fauces, and tongue were excoriated in vomiting; the suppuration which was set up in the track of a seton assumed an inky dark-blue colour, which Dr. Apjohn concluded was the result of the action of muriatic acid on the purulent matter. This case recalls to mind the observation of Heller, that his colourless uroxanthine normally present in urine, and probably derivable from hematin, was converted by the action of acids into the blue pigment uroglauclin. In 1860 M. Fordas (*Jour de Chimie Méd.*, Tome IX., 1863, p. 577) laid before the Academie des Sciences his researches upon the colouring matter of blue suppurations which he had succeeded in isolating, after great trouble, in a crystallized form, and which he proposed to call pyocyanine. The pyocyanine presented itself in the form of blue prismatic crystals grouped in various ways, sometimes in the form of a cross or rosette, sometimes as long needles tied in bundles or radiating in different directions from one point. The appearances of Heller's crystals of uroglaucline are precisely similar (*On the Urine*, Vogel and Newbauer, p. 45). After having isolated pyocyanine he continued his observations on two most protracted cases of blue suppuration in the Charité Hospital, under M. Velpeau, and, in addition to pyocyanine, has obtained also a yellow colouring matter, which he calls pyoxanthose. This discovery of a blue and yellow

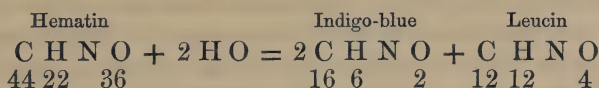
colouring matter in suppuration may throw some light on the simultaneous discharge of the two pigments in Dr. Purdon's case, and a combination of the blue and yellow matter may point towards the explanation of such green colourations as are seen in the pus of cerebral abscesses, in the deranged menstrual secretion, and in conditions of the lochial discharge. Green urine has been seen in Bright's disease, peritonitis, cystitis, and spinal disease; the fluid in eczematous vesicles, at first pale, has been observed to become green by exposure to the air; and in the same case, the urine, slightly green when passed, became much more deeply tinted afterwards (Parkes, *op. cit.*, p. 201).

Blue pigment has been observed in the urine as well as in the perspiration, and in purulent matter; Prout sublimed the blue urinary pigment, and recognized it to be indigo blue. Other observers repeated his experiments, particularly Dr. Hassall, who showed that in many urines the indigo blue could be produced by exposure to the air. Schunck next procured it from the colourless indican, which seems almost identical with Heller's uroxanthine; and the elementary analysis of Kletzensky shows the very closest similarity in theoretical constitution between the two—between indigo blue and uroglaucone, the latter being, with indigo-red, one of the products of oxidation of uroxanthine. Milk, finally, has been found tinged with blue (*Edinb. Vet. Rev.*, 1863, Vol. V., p. 656). The milk of some cows having assumed this colour, M. Mathieu, veterinarian at Sévres, made several experiments to ascertain whether the phenomenon of blue milk depended on the cow or on the dairy. In one case, where a single cow was kept, the milk of which showed this peculiarity, new dishes were employed, and still no amendment took place. M. Mathieu drew milk into two new vessels, and placed them in a dairy; he then drew milk into two glass bottles, stopped them carefully, took them to his residence, six miles distant, and placed them in a chamber, the temperature of which was 15° cent. Three days later blue spots showed themselves on the surface of the cream in the two glass vessels; and after another three days having occasion to visit the dairy where the milk had been set at the same time, he found that it too was covered with blue spots. In another case two cows were kept, one black, the other white, the milk was always mixed, but soon became covered with blue spots, which interfered with its sale. He then caused the milk to be placed separately in two new vessels, drew some

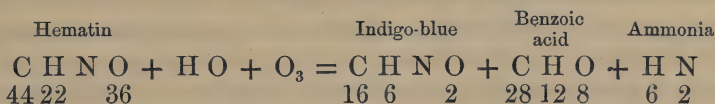
into glass bottles as before, took them home, and placed them in a room different from that of the former experiment. On the fourth day the milk of the white cow was covered with blue spots, while that of the black retained its original colour. These experiments were repeated twenty times, with the same results invariably, from which he concluded that the phenomenon was due to a morbid condition of the secretion in the cow. It is open to consideration whether the blue coloration of the milk was not due to indigo-blue, the result of oxidation of uroxanthine. It is known from Sir H. Marsh's observations that the biliary pigment may simultaneously tinge the urine and the secretion of the human mammary gland; and if one pigment, a yellow one, derived from hematin, may appear in both the renal and lacteal secretions, the same privilege may be accorded to another pigment, a blue one, also connected with hematin.

Dr. Hassall's communication to the Royal Society on the frequent occurrence of indigo in human urine, and on its chemical and physiological relations, was made in 1853. Some of the conclusions which he arrives at are, that indigo-blue is frequently found in human urine, the quantity being subject to the greatest variation; in some cases a pellicle of nearly pure indigo extends over the entire surface of the liquid, while in others it is so small that it can only be detected by means of the microscope. For the formation of this indigo it is in general necessary that the urine should be exposed to the air for some days in an open vessel, oxygen being absorbed, and the blue pigment being developed. Whatever, therefore, facilitates oxygenation, as free exposure to light and air, warmth and sunshine, hastens the appearance of the blue indigo; hence, in Summer the changes described take place much more quickly than in Winter. On the contrary, these changes are retarded, or even stayed, by exclusion of the atmosphere. Blue indigo may even be deprived of its colour, and reformed alternately, according as air is excluded from or admitted to urine containing it. From some of the cases recorded, it would appear, however, that blue indigo is occasionally found in the system, and is voided as such in the urine. There is usually found with the blue indigo, when the amount of this is considerable, a brown extractive, sometimes in large quantity, the aqueous solution of which, by further exposure to the air, yields a further supply of coloured indigo, and which clearly resembles hematin in its chemical manifestations and elementary composition. There is, therefore, great reason for believing that in the majority

of the cases recorded, the blue indigo was derived from altered hematin, although it is at the same time probable that it is in some cases formed from modified urine-pigment, which itself is also regarded as a modification of hematin. Between the greater number of the animal colouring matters there is the closest relationship in chemical composition, so that the transformation of the one into the other would appear to be both easy and natural. Although the exact steps of the conversion of hematin into indigo cannot yet be stated, some hypotheses have been suggested as worthy of mention (Parkes, *op. cit.*, p. 199)—thus, one equivalent of hematin and twenty-two of water will give the elements of two of indigo-blue and one leucin:—



Another formula is, that hematin with water and oxygen will give indigo-blue, benzoic acid, and ammonia:—



Though putting no more faith in these hypothetical reactions than any conjectural formulæ in organic chemistry deserve, yet I think that after considering them we may be in a better position to approach the remaining forms of chromidrosis, which from their local characters and striking peculiarities have always excited much attention. It is a subject of congratulation that we are indebted to the acute observation of Irish physicians for a large proportion of the most remarkable cases of cutaneous secretion of pigment that have been put on record—Dr. Law, Dr. Neligan, and Dr. Banks, in Dublin, and Dr. Purdon and Dr. Reade, in Belfast, have made most valuable communications on this subject.

The case recorded by Dr. Banks was an unmarried female, aged twenty-three, the daughter of a fisherman, and she resided at Skerries, in the County Dublin. She was engaged to be married to a man who was hereditarily predisposed to insanity, and who committed suicide by hanging himself shortly before the time fixed for their marriage. She went to her lover's cottage, and her mother sent her into the room in which he was suspended. She

knew something dreadful had happened, but did not know exactly what it was, until, on rushing into the chamber, she struck suddenly and violently against his lifeless body. She became maniacal, with suicidal propensities, and three months after had to be committed to the Richmond Lunatic Asylum, where, for upwards of two years, her state underwent little or no change. In the meantime she had suffered from deficient menstruation and periodic hemoptyses.

When Dr. Banks first saw this woman his attention was attracted by a remarkable discoloration round the eyes, but chiefly beneath and extending to the side of the nose; and he states the appearance to have exactly resembled what one would imagine to have been produced by painting the part with Indian ink, or perhaps, he says, rather with Prussian blue. The discoloration round the eyes had existed since the cessation of the catamenia, but became of more vivid hue during the continuance of the periodical hemoptyses. She continues still to present this discoloration, but in a slighter degree.

Laycock (*Med. Ch. Rev.*, No. LIV., p. 464) mentions a very similar case in a woman, aged twenty-four, affected with melancholia, and who became the inmate of an asylum. She presented a symmetrical discoloration of the face; but the eyelids, both upper and lower, were, in particular, deeply tinted with pigment. The influence of emotional disturbances of the nervous system in the production of pigments is very strange. The effect of fright upon the colour of some reptiles, as the chameleon and frog, has been shown, by Mr. Lister in a communication to the Royal Society, June, 1857, to be due to the influence of the nerves distributed to the skin.

Another case of blue discoloration was observed by Dr. Billard, in a girl of sixteen, who presented, on the face, neck, and upper part of the chest, a beautiful blue coloration, extending chiefly on the forehead, the *alæ nasi*, and around the mouth. When the face was rubbed with a white cloth, the cloth became stained with the colouring matter, leaving the skin of its natural hue. This girl had menstruated for two years, and there had been no deficiency of this secretion. The blue appearance round the eyes presented itself when she worked in a confined and hot place. At the end of a year the whole forehead and face had become blue, so that the attention of persons who met her was attracted. After the use of certain medicines, an abundant perspiration was observed, and simultaneously a sensible diminution of the urine, and a considerable increase of the blue colour. The intensity of the blue

coloration was influenced by the activity of the perspiration, which stained her linen blue. The blue secretion was submitted to analysis; alcohol and ether easily dissolved it, and evaporation of the solution left a residuum of a blue colour; concentrated HCl produced no change on the stained linen; concentrated NO_5 destroyed the colour; slightly alkaline warm water decolourized the linen; no indication of iron could be detected in it (*Archives Gen. de Med.*, Tome XXVI., p. 453). Again the observation of Heller suggests itself, that in another secretion the urine, in many ways analogous, and also closely correlated to that of the skin, the blue colouring matter, uroglaucoin, was produced by simple exposure to the air; from his colourless principle, uroxanthine, and the close connexion of both of these substances in elementary composition to hematine is to be borne in mind. The great richness of the colouring matter of the blood, of uroxanthine, and also of uroglaucoin in carbon, may assist in explaining the occurrence of this coloured secretion at catamenial periods. According to Andral and Gavarret the maximum secretion of CO_2 occurs at this period in the female organism; and if the catamenial discharge is to be regarded as a mode of eliminating carbon, and that means of secretion is interrupted, the formation of CO_2 being constant, we will be less surprised to find the skin engaged more actively than usual in the performance of that duty. It may be in this direction we are to look for an explanation of the cutaneous discolorations in gout and rheumatism. I have lately seen all the swollen phalangeal articulations of a gouty subject so dusky a brown as to attract attention at once; the colour was deepened during an intercurrent attack of pericarditis. In seven cases of chronic rheumatism, and cardiac disease of rheumatic origin, Laycock (*Med. Chir. Rev.*, No. LIV., p. 475, 1861) observed pigment deposit on the skin, together with more or less pigment in the blood. His explanation is, that if the supply of oxygen to the system be not sufficient for the transformation of lactic acid, $2\text{HO C}_{12} \text{H}_{10} \text{O}_{10}$, (assuming that the phenomena of rheumatic fever are due to the presence, in excess, of this acid) into carbonic acid and water, and it undergoes no further changes, then it appears in the urine; but if the carbon of the flesh be not oxidised into lactic acid, then it is excreted as colouring matter in some form or other.

The next case which I shall refer to is one of five published by Leroy de Mericourt, which exhibits a transmission from blue to black pigmentation. The subject of what I call the transition-case

was a woman in her twenty-first year, of good constitution, whose catamenia had always been regular, and in sufficient quantity, from the age of seventeen. Without appreciable cause, and three days after the cessation of the catamenial discharge, she perceived that the under eyelids had assumed a *blue* colour, which on the following day became of a deeper tinge, closely resembling Indian ink. The discoloration was not confined to the lower eyelids, but extended a little to the cheeks. The colour was rendered more intense by heat, exertion, emotion, or fatigue, which are also causes promotive of the perspiratory secretion; it persisted during the night, and soon acquired the hue which it retained during the day. When examined under a magnifying glass the colour was found chiefly accumulated in the areolar grooves—the seat of the most marked depots were the folds of the lids. It had the appearance as if the girl had passed some time in an atmosphere charged with coal dust, and had washed her face, except the under eyelids. The colour was due to a multitude of little black grains deposited as an efflorescence on the surface of the skin. On rubbing the abnormally-coloured parts with a white cloth it is stained black; but neither by this nor by washing was it possible to restore the integuments to their ordinary colour—the shade was merely rendered less intense, and soon became as deep as before. This may be explained by the precipitation of part of the pigment in the superficial layers of the true skin, of which we have examples in the precipitation of metallic silver, in a state of minute division, from its soluble salts, after the protracted use of that metal (J. Van Guens, *Archiv. für die Höländischen Beiträge zur Natur und Heilkunde*, 1857, Band I., Heft I. und II., p. 106), and as is seen in the occasional fixation of colouring matter in the skin, observed after exposure to solar light or artificial heat, in the various degrees of sunburn, or the *tâches de brulure* on the legs of old persons who sit much before the fire.

The colouring matter of the skin of the negro is deposited in the deepest and youngest layers of the epithelium; it was removed from the skin and isolated by Beclard, by macerating the skin of the scrotum, which is peculiarly dark in the coloured races; the rete mucosum, in becoming resolved into a kind of mucosity, tinged the water, and allowed an impalpable brown powder to fall to the bottom of the vessel. He believed it to be very similar to the colouring matter of the blood, and quotes the chemical observations of Davy, Coli, and others, as having demonstrated what Blumenbach had long ago advanced, that the pigment of the skin is highly

carbonaceous (Beclard, *Elements of Gen. Anat.*, trans. by Knox, p. 146).

Le Cat, a surgeon at Rouen, examined chemically the colouring matter of the cuttle-ink, and showed its identity with that of the skin of the negro and of the choroid coat of the eye. The conversion of hematin into pigment seems to be effected by the influence of agents addressed sometimes to the hematin itself, sometimes to the iron it contains. Rokitsansky cites the effects of strong mineral acids, as the sulphuric, which, introduced from without, darken and blacken the blood with which they come in contact. Carbonic acid gas (evolved, along with carbonic oxide gas, out of glowing charcoal) acts in the same manner upon the capillaries when a stream of it traverses the fauces, and a similar influence is exercised by acid secretions generated in the organism itself, as was seen in the case above referred to, and observed by Dr. Croker, and in the coloured softening of the stomach. The frequent conversion of red hemorrhagic exudates upon the peritoneum into black strata is most probably founded upon the influence of the intestinal gases upon the hematin. In common with ammoniacal gas it is principally the sulphuretted hydrogen of the bowel which, acting by exosmosis upon the iron of the hematin, enters with it into a black combination, namely, sulphuret of iron (*Pathological Anatomy*, Vol. I., p. 211).

In 1802, Dr. Marcet (*Med. Chir. Trans.*, Vol. XII., p. 37) saw a young woman who was subject to attacks of cutaneous discoloration; the colour was black or dark purple; the toes, legs, hips, and face were in succession liable to these attacks. The cutaneous affection usually began with a tingling of the parts, soon followed by a considerable swelling or puffiness over an extent of several inches, which lasted for some hours, and was ultimately followed by the appearance of the pigment. Her urine acquired a black colour during daily paroxysms partaking both of the febrile and hysterical character. This black urine was not investigated at the time by Dr. Marcet, but twelve years afterwards he observed this curious phenomenon, black urine, in a subject who certainly would not be accused of any hysterical disposition, at least in the ordinary sense of the word. This black urine was passed by a healthy male infant of seventeen months. Almost immediately after its birth it was observed that the child's urine tinged his napkins of a dark purple hue, which alarmed his parents very much at first; but as the infant's health remained perfect the alarm subsided, although

this peculiarity never abated for any length of time. When the child had reached the age of about nine months, and when the urine could be more easily collected, it was found that, though perfectly clear on being first discharged, it assumed in a very short time a dark colour, somewhat like that of port wine, which became darker by standing; it was then quite black and opaque, without any sediment or turbidness; on the surface, when examined in a strong light, a dark purplish hue was discernible, giving the liquid the appearance of a strong solution of liquorice. There were occasional variations in the degree of coloration, and it sometimes even totally disappeared; it was observed to prevail in the greatest degree when the child's bowels were confined. A specimen of this urine, quite clear when passed, sp. gr. 1022, was at the end of two days quite black, had an ammoniacal smell, and was sensibly alkaline; after an interval of six weeks it remained precisely in the same state, and after a lapse of seven years was still perfectly unaltered, having preserved its colour, having deposited no sediment, and possessing the same ammoniacal pungency, without any distinct urinous smell. No red globules could be detected in it, in a microscopic examination of it made by Dr. Wollaston; no iron was detected in the black deliquescent residuum left on evaporation.

This urine was further examined by Dr. Prout; the addition of dilute acids produced no immediate change of colour in it, but after standing some time a black precipitate slowly subsided, leaving the supernatant fluid transparent and but slightly coloured.

Schilling mentions the case of an infant, with previous hyperemia of the skin, whose entire surface was covered with a lamp-black matter, which came off with the linen (*De Malanosi*, p. 32. 1831).

The remarkable drawing which I hold in my hand is the portrait of a young lady, fifteen years of age, by whom black matter was voided in the urine and feces, by vomiting, and also appeared on the face. The right half of the forehead represents the discoloration of two days' duration, and the opposite side of five, showing the difference in quantity of the black matter secreted during the longer interval. She was a patient of Dr. Read, of Belfast, and, having been brought to London, her case attracted so much notice that it was communicated to the Medico-Chirurgical Society by Sir B. Brodie (*Med. Chir. Trans.*, Vol. XXVIII., p. 611. 1845). The discoloration commenced on the left under eyelid, near the internal angle of the eye, appearing at the commencement as a brownish spot, which in the course of four or five days assumed a

jet black colour, and gradually extended to the entire forehead and eyelids of both eyes. The discoloration never appeared on any other part of the body, and on the forehead was accurately limited by the hair. Attempts to wash the black matter off caused so much pain, from the sensitiveness of the skin, that she discontinued to do so. The quantity of black matter removed on one occasion by soap and water was sufficient to darken four basins of water as black as Indian ink. Some letters of this lady's father to Dr. Read show the state of perplexity her medical attendants were in to account for the phenomenon:—"They were so much at a loss," writes her father, "that they fell into a curious mode of solving their doubts, and declared the child had rubbed some black stuff upon her own face, for some causes to them (and to me, I declare) unknown. This took such firm hold of their minds that, having met, they called for warm water and soap, with which they certainly removed all appearances of blackness from her countenance, leaving the skin perfectly smooth and unruffled. This operation, you may well imagine, only tended to confirm them in their strange opinions, and I had to sit one whole day watching her most narrowly, and locked her up at night by herself, before they would believe that some imposition had not been practised. However the discoloration returned in the course of five or six hours; and, fortunately for the sake of the poor child's character, it came on gradually, in the presence of Major Greenwood and several others, while we sat at the dinner-table." On a subsequent occasion her father writes:—"The medical men still entertain the supposition of a trick—at least they were so sceptical that one of them came on Monday last, and, after washing all traces of it off from her face, remained with her eight hours, when, from his own observation, he was satisfied it proceeded from natural causes; and thus, I trust, all these nonsensical doubts are for ever set at rest."—(*Dub. Med. Prsses*, Vol. XV., p. 204).

It is admitted by Mr. Teevan that this patient had no tendency whatever to hysteria; the catamenia were regular, though scanty; during the period when the black matter was ejected from the stomach, bowels, and kidneys, there was a cessation of its secretion by the cutaneous exhalents of the forehead. Examination of the black matter from the face was made by Dr. Owen Rees, who reported it as composed of carbon, iron (in some unknown form of combination), lime, animal matter (albuminous), fatty matter, alkaline phosphate, and chloride. It was insoluble in water, it

yielded fatty matter to alcohol; the black colour was not changed by the action either of caustic potash or strong nitric acid. When moistened with distilled water it reddened litmus paper. Under the microscope was seen a confused mass, apparently made up of short hairs, epithelial scales, granules, and globules of fat. The black matter vomited was very acid; the carbon from the face was so finely divided that it burned with a vivid combustion, almost having the appearance of deflagration. Carbon, he continues, is certainly the colouring matter of this material (*op cit.*, p. 618). The lady became perfectly cured of this complaint. Dr. Neligan made a communication to the College of Physicians ten years ago on a "Peculiar Black Discoloration of the Skin of the Face" (*Dub, Quart. Jour.*, Vol. XIX., p. 293), which he had seen in a patient of Dr. Quinan's, and a drawing of whom I have in my hand. She was twenty-one years of age, the catamenia were suppressed; and after having for some time suffered from irritability of stomach and periodic vomiting, a dark bluish-black stain appeared at the inner canthus of the left eye, and soon both under eye-lids were discolored, the dark patches extending and becoming of a deeper colour at each catamenial period, the skin was so excessively tender at the seat of the discoloration that she could not bear the most gentle pressure with the finger on the parts, and she would not allow the removal of the colour by washing; the black discoloration subsequently extended very much over the face, engaging both cheeks to below the malar bones, and also the alæ of the nostrils. On examining the dark patches with a powerful lens, Dr. Neligan observed that the stain was dotted over the surface of the skin, the dots corresponding to the orifices of the sebaceous glands, and for this reason he approves of the term *stearrhœa nigricans* for this affection. He considered that, as not unfrequently occurs in females in whom the menstrual function is deranged, the secretion of the sebaceous glands becomes augmented in quantity, and that in these special cases it is further more altered by being stained with the colouring matter of the blood. Dr. Law suggested (*op. cit.*, p. 300) the name *blepharo-melena*, with reference to the case of a patient of his who suffered from a dark areola round the eyes, which subjected her to much persecution and annoyance when living as governess in the family of a homeopathic physician, who, notwithstanding the written testimony of Dr. Law and Sir H. Marsh to the genuineness of the phenomenon, persisted in accusing her of imposition and deceit. In the *Mémoires de l'Académie*

Impériale de Médecine (Tome XVII., XVIII., 1853-4) is a case of black coloration of the face observed by M. Bosquet, in the person of a married woman, heretofore in good health. Upon the subsidence of a violent paroxysmal cough she found her face gradually assuming a darker and darker colour, until it looked as if smeared with a "thick solution of indigo." The colour afterwards became still deeper, until her face resembled that of a negro under slight transpiration. By washing, the black layer could be removed, the skin then presenting a bluish-black colour, as the chin of a person having a strong black beard does after shaving. After a while small drops of black fluid appeared at the surface, and forming into a compact layer, gave the same appearance as before. Linen coming in contact with the face acquired stains that repeated washing only effaced. The colour, which had commenced towards the end of August, had quite disappeared by the middle of December, the only symptom noticed during its persistence was severe headache. With reference to this flux of facial pigment the question arises whether its appearance on the free surface of the skin is in accordance with the physical law of exosmosis, which the ordinary cutaneous secretion obeys, or whether it is the result of a capillary rupture; there are grounds for believing that the black, blue, or bluish-black colouring matter is a secretion from the capillary vessels of the skin, especially those of the sudoriparous glandular coil, because it is not observed that blood cells are present, or any structural constituent which would violate the integrity of the vascular membrane, and from the fact that in the cognate secretion, the urine, there is a recognized soluble principle, colourless at first, which can afterwards become blue or black, and then admits of precipitation and collection; there are also cases in which the dark pigment exists preformed in the blood, as in the cases of melanemia observed to accompany enlargement and pigmentous deposits in the spleen (Virchow, *Cell. Path.*, p. 222).

Laycock believes that the morbid pigment deposits proper, as distinguished from masses of altered blood corpuscles, are carbonaceous excretions, and are often vicarious with the suspension or imperfect elimination of other carbonaceous excretions—as the carbonic and lactic acids, and the pigment constituents of the urine and the bile; he properly draws a distinction between carbonaceous secretion and carbonaceous deposit, the examples of fixation of

carbon on the surface of the body are much more numerous than of its excretion as a dark pigment.

The secretion of colouring matter on the face is almost peculiar to females; in its commencement it is to be distinguished from the venous lividity of the lower eyelids observed during menstruation, and from the blueish hue derived from the circulation of imperfectly aërated blood. Its preference for the face, if not connected with the free and full exposure of that part of the body to light, heat, and air, great colour-developing agencies, may be referred to that law of selection which leads to the pigmentation of the mammary areola in pregnancy, or even in ovarian disease without pregnancy, and to the formation of the dusky abdominal streak observed under the same circumstances. These cutaneous pigmentations—the utero-ovarian melasma of Laycock—are so constant in pregnancy as to become diagnostic of that condition, but they are not confined always to the same part of the body. Montgomery^b (p. 283) alludes to a lady of rank, who, when pregnant, was much annoyed by the appearance on her forehead and nose of two streaks, exactly resembling the marks that would be made by very dirty fingers drawn along these parts; another lady in her first pregnancy observed brownish spots on the sides of the forehead and temples, which she at first mistook for accidental soils on the skin; they remained permanent; further pregnancies made additions to them, so that at last she was obliged to dress her hair in a particular manner to conceal them. Le Cat and Camper have observed the face become quite black in pregnancy. Discoloration of the lower eyelid has been observed in men; Laycock (*Med. Ch. Rev.*, No. LIV., p. 467) remarks that this blepharal melasma, as he calls it, usually commences on the inner margin of the lower eyelid, and extends like a daub of Indian ink from thence to the middle of the lid; he observed it most commonly in men with dusky complexions, of enfeebled health, and nervously disposed; though it is not easy to see how far it is in relation with the genito-urinary system, he thinks it was so in the three or four cases he noticed. The actual transudation of the pigment from the blood vessels seems to be dependent upon conditions of the vaso-motor nerves; the generally symmetrical nature of the deposit, the absence of local disease of the skin, the great hyperesthesia of the seat of coloration, all point to lesions of the innervation of the integument, causing local

^b Signs and Symptoms of Pregnancy.

determinations of blood and local variations of vascular action. Dr. Neligan has expressed his opinion that this colouring matter is derivable from hematin; Professor Laycock is in favour of its carbonaceous origin. In conclusion, I am anxious to direct attention to the connexion between many anomalous appearances of colour in various secretions and the formation of indigo compounds; from the facts that the coloured exudation on the skin has been in several cases blue all the time it continued, in others blue at first and becoming black, perhaps only from intensification of the original colour, that we are not acquainted with any blue form of carbon, and that there is a colourless substance normally present in the urine, from which indigo blue is produced by mere exposure to the air.

ART. VIII.—*Clinical Reports of Rare Cases, occurring in the Whitworth and Hardwicke Hospitals.* By SAMUEL GORDON, M.B., F.K. & Q.C.P.; Physician to the Hospitals, and Lecturer on the Practice of Medicine in the Carmichael School of Medicine.

(Continued from Vol. xxxiii., page 356.)

Most Extensive Pleuritic Effusion Rapidly Becoming Purulent; Paracentesis; Introduction of a Drainage Tube; Recovery; Examination of Interior of Pleura by the Endoscope.—I do not propose entering upon the history or details of the operation of *paracentesis thoracis* and the introduction of drainage tubes, but simply to record another case of recovery by this mode of treatment, and which has taken place under very adverse circumstances. I am induced to do so because I believe that this mode of treatment, originated by Dr. Goodfellow, of the Middlesex Hospital, has not met with sufficient favour on this side of the Channel.

In this city Professor Banks, Dr. Kidd, and myself have from time to time laid before the profession cases in which it has proved successful, and what is of more importance, cases whose recovery we would have despaired of if they had not been so treated.

It is not long since I had under my care a boy, about fourteen years of age, son of a physician of considerable repute in his own district. This boy upon recovering from a very severe attack of malignant scarlatina, was seized with acute pleuritic inflammation of the left side, with effusion of purulent matter to a large amount;

the case presented all the usual phenomena of a large empyema of necessity, and pointed between the third and fourth costal cartilages, and very close to the sternum. The external abscess was opened, and over a pint of purulent matter escaped; from day to day a large quantity was still drawn off by changing the patient's posture, making him cough, &c., &c.; but it was distressing to find how these same exertions, so exhausting to the patient's strength, had to be continually employed to check the accumulation of the fluid. Under these circumstances, I proposed to his father to introduce the drainage tube, in the way proposed in these papers to which I have alluded to; but he did not consider that that operation had been shown to be so advantageous as to warrant his submitting his child to a second incision into the pleura, an operation which he looked upon as in many ways most hazardous.

The boy gradually sunk from an exhausting purulent secretion, which was retained within the system. In my opinion, had the operation been performed he would, in all human probability, have made a good recovery.

I look back on this case with regret, and I have to look back with a similar feeling on some others, which I can but too well remember, and which had a similar termination, but which I believe would have had a far different ending had this means of relief been at that time known to us. Impressed with this feeling, I wish to record the following case, which I think testifies in a remarkable manner to the value of this mode of treatment.

Anastatia Baily, a very delicate-looking little girl, eleven years of age, was admitted into the Whitworth Hospital on the 27th of April last, under circumstances of urgent respiratory distress.

The *history* which her mother, who carried her into hospital, gave of her illness was, that she was always a healthy child until about two months ago, when, for the first time, she began to complain, principally of a more or less constant pain of the left side and a slight catching cough. The cough had of late become much more severe; but the urgent symptoms under which she now labours did not attack her until about a week ago, when the pain and stitch in her side became much augmented, and when she became attacked with gradually increasing oppression of her breathing. She has had no definite rigor, though she has often complained of chills. She had been in bed for ten days previous to admission. No history of family scrofulous taint could be made out.

After admission, when examined as she lay in bed, it was observed that the decubitus was diagonal, midway between dorsal and that on the left side; the respiration was obviously much laboured and hurried—68 per minute; pulse 140, and very weak; she made much complaint of pain, tenderness, and stitch in the lower part of the left side; cried out much on being moved or asked to move, and became much more oppressed when raised into the sitting posture or made to lie on the right side.

The *physical signs* were those of left pleural effusion in a very exaggerated degree. The displacement of the heart immediately attracted attention; its apex plainly pulsated an inch to the *right* of the margin of the sternum, and half an inch *higher* than the nipple. On percussion the whole left side of the chest was absolutely dull from top to base; the dulness, moreover, encroaching on the other side beyond the right margin of the sternum, and the spines of the vertebræ being quite dull behind. The left side looked much dilated, and measurement at the level of the nipples showed it to exceed the circumference of the right by three-quarters of an inch; it presented both to the eye and hand a perfectly smooth surface, and the natural depression of the supra-clavicular fossa was quite filled up.

To the dull region corresponded absence of all respiratory or vocal sound, except over the upper part of the sternum, and in the left infra-scapular region, in which parts bronchial breathing existed. The breathing over the right lung was intensely puerile. Some enlarged superficial veins were observed traversing the front of the left side.

The remedial means adopted, which consisted in the application of two leeches to the side, and covering it with a light warm poultice, and the exhibition of small doses of mercury and opium internally frequently repeated, failed completely in promoting the absorption of the effused fluid; on the contrary, it appeared to have increased, and on the following morning the dyspnea had become much more urgent and decided evidences of insufficient aëration of the blood existed. The face and surface generally were somewhat livid; her strength was evidently sinking, and she complained more than ever of her side. Pulse 144; respiration 66.

The operation of thoracic paracentesis was performed on the 29th of April. The puncture being made between the third and fourth ribs, at a point near the sternum, where the integument was thinned, and where there was a decided impulse on coughing. Exactly a

quart of clear greenish-yellow serum was drawn off. I used Bowditch's syringe on this occasion; but despite all precaution, a considerable quantity of air entered the pleural cavity. During and after the operation the lung appeared to make a slight effort to expand; feeble respiratory murmur became audible behind and above, but not in front. The heart decidedly moved much towards the left, and immediately after the operation had nearly gained the region of the ensiform cartilage. The wound was immediately closed with adhesive plaster.

Two hours after the operation.—The patient had slept quietly since the operation, and her respiration had become greatly relieved. Pulse 128; respiration, 44. No examination of the chest made.

In the evening the pulse had risen to 136 and the respiration to 52. A tympanitic condition of the upper part of the left side showed that a corresponding portion of the pleura was occupied by air. The heart had returned to the position it occupied before the operation.

April 30.—Has been very restless and did not sleep last night. Pulse, 140; respiration, 68.

May 2.—Her weakness is extreme, and her dyspnea most urgent; her *alæ nasi* widely dilated, and her pulse scarcely perceptible. Symptoms of accumulation have gradually progressed, and to-day the symptoms of pulmonary oppression were as marked as on her admission. Her lips were blue; her pupils dilated; the heart was even more displaced than at first, being now close under the right clavicle. Pulse, 144; respiration, 72.

The chest was again tapped in the same situation; but when I found that purulent matter was escaping through the canula, I withdrew it, and closed the wound, and looked for the most depending spot in which I could safely open the pleura—the heart being under the right clavicle and the diaphragm convex downwards, I had not much difficulty in finding and opening the intercostal space. Twenty-eight ounces of purulent matter were drawn off; a *drainage tube* was then introduced through the canula, and left in the chest; the canula being withdrawn.

By this second operation she was much relieved, though not to the same amount as on the first occasion. She appeared to have less vigour of reaction than before; and she was obliged to be supported by large quantities of wine.

Three hours after the operation.—She slept after a small opiate.

Pulse, 140; respiration, 56. There is but a very trifling discharge through the tube. The air freely plays in and out of the pleura through the orifice during each act of respiration. The heart has not in the least returned from its abnormal situation.

May 4.—Pulse, 126; respiration, 52. Discharge from the tube very trifling. She is so weak as to require a large glass of wine every hour.

10th.—Since last report the purulent discharge has greatly increased. The patient has much deteriorated in aspect; she is now very delicate and *hectic*-looking, and seems to have wasted considerably. She sleeps constantly. She is taking bark, and chlorate of potass, and large quantities of wine. The measurement of the two sides makes them about equal.

17th.—Still looking extremely delicate and hectic. Copious and very fetid discharge flows constantly through the tube; a large quantity (about five ounces) came away to-day upon the removal of the old and introduction of a new tube. The pulse has remained high, varying from 120 to 132. For the last few days she has been ordered cod-liver oil, but she obstinately refuses to drink it, because it invariably makes her sick, even in very small doses, and at very long intervals. I therefore substituted for it fresh mutton suet, dissolved in boiling milk, as much as could be taken up, and of this she drank freely day and night, taking at least two quarts of the milk so saturated in the twenty-four hours. She had some tendency to diarrhea, which was generally checked by small doses of the pernitrate of iron; she still required eight ounces of port wine daily, and occasionally some syrup of bark. This treatment was continued for two months, and on the 21st of July, on my return after some weeks' absence, I was agreeably surprised to find her wonderfully improved in every respect. She was able to be up every day. She had become quite bright and healthy looking. The left side had become wonderfully contracted; the shoulder depressed—a marked depression corresponding to the left infra-clavicular region. The diaphragm had now returned to its normal position. The right side, on the contrary, had become much expanded, and moved exaggeratedly during respiration.

A remarkable system of large subcutaneous blue veins had become developed over the front of the chest, more especially on the left side. The heart was still under the right clavicle; a small quantity of pus escaped daily through the tube.

During the next two months there were occasional relapses

of diarrhea and of mild attacks of bronchitis of the right lung, but she was seldom confined to bed for two consecutive days, and her general health progressed favourably. The left side of the chest was gradually contracting and the right enlarging. On the 1st of October there was a difference of two inches by measurement at the level of the nipple, and the antero-posterior diameter showed a difference of two and a quarter inches. There still existed a most remarkable net-work of large superficial veins over the upper part of the sternum and left side of the thorax; at this time the contraction of the left side of the chest appears to have been at its maximum, and the child's general health being greatly improved, the hitherto contracting side from this time began to enlarge.

From this time her progress has been almost uninterrupted. On the 15th December, the smallest sized tube was introduced, and was finally withdrawn on the 13th of January. The fistulous orifice remains, and occasionally a drop of thin pus exudes from it. The sternum is bent obliquely downwards, and to the right side, the manubrium being drawn over to the left side. The infra-clavicular space is still much depressed; but, excepting this portion, the left side has fully expanded, and the intercostal spaces have again assumed their normal width. The varicose veins have almost disappeared from the front of the chest, and the heart has moved downwards, and towards the left side, being under the sternum, but not having yet passed its left border.

This case was evidently from the beginning one of very acute inflammation of the pleura, and urgently requiring thoracentesis. The fluid evacuated being serum, the wound was, of course, in the first instance, carefully closed. The extreme rapidity with which the change from serous to purulent fluid occurred is worthy of observation; it prevented one entertaining any well-grounded hope that there would not be a further re-accumulation; the opening was, therefore, in a most unfavourable situation for the constant evacuation of accumulating fluid; and I considered that there were only two plans which could be followed—either to complete the evacuation as far as possible through the opening now made, and subsequently to introduce a drainage tube, with a counter opening lower down, according to Dr. Goodfellow's plan—or, as I did, to close the original opening, and at once introduce a tube at the very lowest part of the pleura. By the latter proceeding I avoided having two pleural fistulæ where one would suffice, and at the same time effectually prevented any accumulation of pus in the

pleura. To this curative means, which the drainage tube so effectually accomplished, this patient's recovery, in my opinion, is principally owing. The case proves, also, that one insertion of a tube is sufficient, provided it be sufficiently depending, and that, therefore, Dr. Goodfellow's plan may be so far modified that, when it becomes necessary to make a second opening lower down, the upper orifice may be allowed to heal.

I never experienced any difficulty in keeping the tube in the chest, or in re-introducing it when I deemed it necessary to change it. Although these are the two principal facts to be learned from this case—1st, the advisability of treating empyema by means of drainage tubing; and, secondly, that the insertion of the tube through one depending opening is sufficient under ordinary circumstances. There are other circumstances in this case deserving of some attention.

I have seldom seen a case in which the administration of cod-liver oil seemed more clearly indicated; but, besides that the patient seemed to have a natural repugnance to it, the stomach always rejected it, even in the smallest quantity, we were, therefore, reluctantly obliged to abandon it, but fortunately were able to find an admirable substitute for it in the mutton suet, and I can confidently recommend its use in similar cases.

The enlarged and varicose condition of the veins in parts of the chest was for a long time very remarkable. I have seldom seen them more enlarged in cases of aneurism or extensive malignant disease. I considered that they were caused by the lymph which had been effused undergoing the usual process of contraction, and that this had extended to the subserous structures, and so involved the internal mammary veins and others, passing up to the left innominate, and probably a portion of this trunk itself; afterwards, when the side began to expand and the ribs again to separate, this superficial venous distention gradually diminished, and has now almost entirely disappeared.

This gradual enlargement of the diseased and contracted side is perhaps one of the most remarkable and important features in the case, and should never be lost sight of in the prognosis of empyema. We have proof here, how even when the side is reduced almost to a minimum, the process of expansion may again take place, and the lung be restored to a useful and comparatively healthy condition.

The permanent dislocation of the heart is another feature of interest. It is now pushed slightly downwards and inwards from

the place which it occupied beneath the right clavicle; owing, I believe, to a mechanical pressure on it from the enlarging right lung; but I think it probable that on account of the amount of adhesion which has probably taken place in the pleura, immediately adjoining the mediastinum, that the heart will never resume its normal position in this patient.

Another very interesting point in this case is, that although there is no sign of phthisical development, yet the hand is as good an example of what is termed the clubbed fingers as can often be seen. I believe myself that this condition is as often, if not more often, associated with chronic pleurisy than with chronic phthisis.

This case is also very remarkable as being the first in which an examination of the interior of the chest has been made by the endoscope. The facts which we satisfied ourselves of were that the lung was not compressed towards the spine, nor much reduced in size; that is that there was no large vacant pleural cavity, and that whenever we saw the pleura it presented more or less a granular surface, and no appearance of lymph.

To ascertain the condition of a lung in a case where a pleural fistula exists can never be a very easy matter, an endoscopic examination may in such cases, therefore, be most valuable—that it is perfectly practicable this case proves.

I am indebted to Dr. Cruise for making the examination. It was the first occasion upon which he had used the binocular adjustment, which renders the instrument so much more perfect.

CASE II.—Case of Reno-Pulmonary Fistula—Ulceration of the Duodenum.—Mary Healy, aged twenty-four was admitted into the Whitworth Hospital on the 18th of January, 1865, labouring under well-marked symptoms of hectic fever. She had profuse night perspirations, profuse purulent expectoration, and diarrhea, and during her stay in hospital she daily passed large quantities of purulent urine.

The history which she gave of her illness was, that she had been for a long time subject to what she termed “gravel;” for these symptoms of urinary distress she had never received any beneficial treatment, and they were of about twelve months’ duration; that during all this time her health was gradually declining, her appetite failing, and emaciation rapidly progressing. Within the last three months she has suffered from severe cough, which has distressed her more than all her other ailments; it is so incessant, and adds so

much to the urinary distress, as to cause almost constant stillicidium.

She was excessively pale, anemic, and slightly anasarcaous universally; her voice was excessively feeble. She had no vomiting, but a dislike to all kinds of food; and she was altogether very like a person in the last stage of pulmonary consumption; but on examining her chest there was no evidence of extensive disease in the upper part of either lung; in fact there was no sign of organic disease in any part of the left lung, but in the inferior portion of the right lung there existed the usual evidences of a pulmonic abscess; and from the extent of the dulness, and the intensity of the gargouillement and pectoriloquy, it was inferred that either the abscess was very extensive, or that it was surrounded by a large portion of solid lung. During her stay in hospital there was no important change in her symptoms. From the time of her admission she complained greatly of pain in her right side, and over all the right hypochondriac and lumbar regions there was great diffused swelling, and tenderness on pressure; it also gave her much pain to move, and her constant position was on her back, inclined to her right side. She was kept alive by wine whey, and other light nutriment, and the severity of the cough was somewhat abated, when, on the 8th of February, there was observed a decidedly gangrenous fœtor from her breath and from the expectoration. Although this peculiar symptom is by no means diagnostic of gangrene of the lung in ordinary cases, still, when it supervenes in cases where the existence of a pulmonary abscess has been previously recognized, we need not hesitate to assert that the walls of the abscess have taken on a gangrenous action. In the present case there could not have been any hesitation in the diagnosis; for, in addition to the peculiar fœtor, there was a suddden sinking and collapse of her already emaciated frame, and from this she never rallied. She continued to sink, and died on the 12th of February.

Autopsy (nine hours after death).—On removing the lungs from the chest, some old adhesions on both sides had to be overcome. Those on the left side were few, and easily broken down. On the right side they were in parts very firm, especially those which bound the apex of the lung to the cone of the pleura; the postero-inferior portion of the lung was bound to the thoracic parietes and to the diaphragm by a thick false membrane at least one-fourth of an inch in thickness. When this was stripped off from the lung, it was found to have bounded, inferiorly, a superficial cavity in the lung,

of very irregular shape, and containing probably about four ounces of offensive greyish fluid, and having the usual aspect of chronic pulmonary excavations, crossed by the ligamentous remains of blood vessels, and with several tolerably-sized bronchi gaping into it; its surface was very uneven. The portions of lung tissue bounding this cavity superiorly and laterally were, for a considerable space around it, solidified, and sank in water; there was no trace of disease, tubercular or otherwise, in any part of the remainder of that or of the opposite lung.

The heart appeared to be of normal size, and of perfectly healthy structure.

Abdomen.—The liver was of healthy appearance, but was found to have contracted adhesions to the surrounding parts, as follows :—A small oval ulcer in the commencement of the duodenum had perforated through all the coats of that viscus, and ulcerated its way for a certain distance into the substance of the liver, at the lower edge of the quadrate lobe, forming a small oval depression in that part; the adhesion of the liver to the duodenum in this part was very slight, giving way without the least resistance. The adhesion of the upper edge to the diaphragm was very intimate; on endeavouring to separate the inferior surface of the right lobe from the parts beneath, the sac of an abscess was opened into, which further dissection showed to be one in connexion with, but external to, the right kidney; the cyst of this abscess was, to some extent, formed by a thickened layer of the capsule of the kidney; its boundaries and extent were as follows:—It commenced in the right renal region, and completely enveloped the kidney; it was here bounded on all sides by the distended capsule; it passed directly upwards behind the right lobe of the liver, which formed its anterior boundary, and was itself tilted forwards; the posterior and external boundary was now formed by the diaphragm, pushed upwards to an extraordinary degree; and the cone of the abscess was formed by a remarkably thick deposit of lymph, but which corresponded accurately to a spot on the inferior surface of the diaphragm, which was remarkably thin and depressed, and this depression again corresponded to, and by its superior surface formed the centre of the base of the pulmonary abscess; at this spot the diaphragm was on the point of giving way, but as yet there was no complete solution of continuity.

The intestines were removed with the stomach. The latter organ was of enormous dimensions, but otherwise presented nothing

abnormal. Two ulcers existed in the duodenum, immediately beyond the pylorus. On its anterior aspect was the one already noticed as having penetrated the quadrate lobe of the liver; it was somewhat larger than a shilling. The other, of about the same size, was at exactly the same level, and towards the inner and posterior part of the intestine. It was of exactly the same character, and had perforated the intestine, and penetrated the substance of the head of the pancreas, to which the duodenum was intimately adherent. The edges of both these ulcers were raised and thickened, and hard, almost cartilaginous in feel; they were both plugged with coagulated blood. No other abnormal appearance could be detected in the intestines.

The right kidney appeared to be of enormous size. Its inferior edge extended to a distance of an inch and a half below the crest of the ilium. The abscess connected with its capsule has been already described; it was capable of containing, probably, about twelve ounces of fluid. After this abscess was completely emptied, the kidney remained of immense size, and presented to the touch a distinct feeling of fluctuation; and when the ureter was cut across, a quantity of purulent matter flowed out of it. The kidney was removed, and an incision made into it along its convex border, from which a quantity of pus escaped, and along with the pus a small loose calculus, of peculiar shape. It was round, with a great number of spiculæ projecting from it. Its general surface was of a dirty whitish colour, but the points of the spiculæ were of a dark brown, *i.e.*, the whitish investment was deficient there. Another calculus was felt, imbedded deep down in the pelvis of the kidney, and had to be dissected out, as it was very tightly impacted; it was of large size. Its general shape looked as if it had been moulded to one of the renal infundibula; its surface generally was of a dark brown colour, rough, and with small tubercles over it; in one or two places, where it was broken, it was white.

The structure of the kidney had undergone important and extensive changes. Hardly any trace of its glandular structure existed, and indeed it seemed to exist merely as a thick sort of capsule, enclosing the pus which was contained in its enormously enlarged pelvis. Its appearance when cut into was peculiar; the lining membrane was white, smooth, and thickened; and it presented numerous complicated inequalities, depressions, and elevations.

The left kidney was of large size, but of healthy appearance on section.

The urine in the bladder contained pus in copious quantity.

It is not difficult in this case to trace the progress of the disease. The formation of the renal calculus, and obstruction thereby to the discharge of the urine, was evidently the first step in this vast amount of suppuration. By degrees the entire renal structure was destroyed, partly by the pressure of the distended calyces and pelvis, and partly from disease of the secreting portions. The decomposed urine and the irritation of the calculi induced a process of suppuration, which eventually took the place of the renal structure, and distended the investing wall of the kidney to an enormous extent. How it could have borne such a degree of distention without rupture seems marvellous; it appears to have relieved itself in two ways—first, by the suppuration having ulcerated a passage around the large calculus into the ureter; and, secondly, by the process of endosmosis, by which the renal capsule was to such a degree detached, and so large a secondary abscess was formed, filling the entire right side of the abdomen. We have next to observe the efforts made for the discharge of this vast purulent collection. It appears to me that abscesses connected with the kidney attain a larger size than any other internal purulent collection, excepting perhaps empyema. The quantity of matter in the present case exceeded four pints; and we read, in Monneret and Fleury's admirable compendium, of abscesses of this kind enclosing fourteen pints of pus: they quote also Ballotta's almost incredible case, in which the left kidney presented a circumference of four feet; and the contents of the abscess in and around it—pus, blood, and calculi—measured sixty-eight pints.

We find in this case very clearly established the mode in which a reno-bronchial fistula takes place. It is more than probable that in all such cases the perforation of the diaphragm is the last thing to occur: the details of these cases (there are several recorded) give the history, first of the renal abscess, then of pulmonary disease; and, after that, the sudden expectoration of purulent matter, with a marked diminution of the quantity passed *per urethram*.

It is very remarkable in the present instance that the disease has occurred on the right side. Cruveilhier thinks its occurrence on this side impossible, on account of the presence of the liver; certainly Rayer's case is by no means satisfactory; but Spenser records^a a case which puts the possibility of its occurrence beyond doubt; and the

^a *Vide Journ. des Conn. Med. Chir.*, 1840, p. 122.

present case both proves its possibility, and shows the mode of its occurrence.

The ulceration found in the duodenum at once pronounces the disease there to have been of a chronic character. It was not marked by any symptoms during life except the whiteness of the alvine discharges, and the existence of fatty matters in the stools; these were at the time, however, referred to the hepatic disease.

There are two ways of viewing this ulceration—first, from its contiguity to the large abscess, it might be considered that the pus which had already gained an entrance into the liver was endeavouring to effect a passage also into the intestine. Rayer describes and depicts a very remarkable case of reno-duodenal fistula; and, if there were but the one duodenal ulcer, that connected with the liver, this would probably be the more acceptable explanation; but the direct connexion of the second ulcer with the pancreas would require, at all events, some additional interpretation; we must therefore consider the duodenal ulceration as being altogether a distinct disease, and perhaps even the primary affection; and that the non-assimilation of food caused thereby was the cause more or less directly of the formation of the renal calculi, which eventually caused the death of this patient.

The calculi consist principally of lithic acid, with oxalate of lime.—(Museum, Carmichael School).

CASE III.—Case of Hypertrophy of the Heart from Renal Disease—Mitral Regurgitation independent of Mitral Valve Disease.—

A man about forty years of age was admitted into No. 5 ward, Whitworth Hospital, in February last, suffering from symptoms of renal dropsy; there was also observed at this time a *bruit de soufflet* at the apex of the heart, synchronous with the first sound; and inasmuch as he also suffered from some pulmonary congestion, he was supposed to have disease of the mitral opening. In this diagnosis we were confirmed by the apparently increased impulse of the right ventricle, and the abnormal clearness of its sound. He had a considerable amount of anasarca, and the symptoms of pulmonary distress were also severe; but from both he obtained gradual and decided relief; and after some time he, at his own request, quitted the hospital. He remained out for some time, but returned, labouring under pericardial effusion to an enormous amount, and having also an effusion of fluid into the left pleura. The pericardial effusion was diagnosed principally by the fact, that during his long

previous stay in hospital the sounds of the heart and the impulse were always peculiarly loud and strong, but now there was no impulse whatever, even from a heart which, as shown by the *post mortem* examination, was very greatly hypertrophied. From this pericardial effusion, however, the patient recovered to a very great extent; so much so, that on the *autopsy* there was only a very small quantity of fluid found in the cavity of the pericardium. Before his death the impulse of the heart, and the sounds which had been inaudible, had returned. He died with a very great effusion into the left pleura; the left lung exhibited the carnified condition of a lung long subjected to pressure, with one or two spots of old pulmonary apoplexy, from which he had before suffered, but which were now forcibly compressed. A quantity of soft lymph was deposited over various parts of the heart's surface; but during life the fluid effusion was never absorbed to such an amount as to allow of pericardial frottement being audible. The heart and other viscera were submitted to the Pathological Society on the 8th of April last.

The principal point of interest in the morbid anatomy of this case is the condition of the left ventricle of the heart. The diagnosis during life was, that it was a case of permanent patency of the left auriculo-ventricular opening; it was not considered to be a case of much narrowing of the opening, because the pulse was never intermitting nor remitting, and was always perceptible at the wrist; there was a loud *bruit de soufflet* at the apex of the heart, not audible at the base, at least not more so than could fairly be attributed to the sound which originated at the mitral opening; this opening, it was evident from the *post mortem* examination, was not in its perfectly normal condition; the valve was very slightly thickened, but there was no puckering nor contraction of its substance; still the edges did not meet, and there had evidently been a regurgitation of blood into the left auricle. The condition of the heart, as shown by the autopsy, was not what we would expect to find if the diagnosis were correct, that the primary disease was insufficiency of the mitral valve; it was rather the condition which we usually find connected with long-continued patency of the aortic opening. It was, in fact, a well-marked example of hypertrophy, chiefly of the left ventricle of the heart. The weight of the heart was increased to thirteen ounces; the left ventricle was elongated, and its walls were nearly an inch and a-half in thickness. At first I was inclined to think that there must have been some

regurgitation from the aorta into the ventricle, but which, from some peculiar cause, was not evidenced during life. Before slitting down the aorta, we put it under a pipe to test the valves, and found the water was perfectly retained; there was, therefore, no insufficiency of the aortic valves; and, on looking at the ventricular surface, we found that the valves met accurately; there was no thickening of them, and the aorta also was sound. We must, therefore, look elsewhere to account for the greatly hypertrophied condition of the left ventricle; it certainly was not produced by aortic disease, nor is it the condition of ventricle which arises from permanent patency of the mitral opening. We are, therefore, driven to find the cause of it in the condition of the kidneys, which we find affected with extreme obstructive disease. I believe it was the first occasion upon which this consequence of obstructive renal disease was demonstrated to the Pathological Society. The idea, however, as to their intimate connexion is by no means novel. Latham, in the year 1846, wrote:—"So, too, of the heart. It will palpitate without apparent cause inherent in itself; it will undergo organic unsoundness, still without cause belonging to itself or to other parts, save what can be inferred from the presence of albuminous urine." I have long thought that one of the most common causes of hypertrophy of the wall of the left ventricle is renal disease. The principal immediate cause of this ventricular hypertrophy is to be found in the obstruction to the capillary circulation produced by the diseased condition of the blood. Dr. Basham, in his late Croonian Lectures, makes the following very clear statement as to this condition:—"It is now generally admitted that in the so-called blood diseases the capillary circulation becomes impeded; so that, whenever the blood is charged with morbid material, or is in any way rendered unfit for the processes of nutrition or secretion, a stagnation or imperfect movement through the capillaries becomes manifest, not only by obvious deviations in the integrity of the functions of the tissue or organ, but especially by the increased frequency of the heart's action, and the laborious efforts made by that organ to force the blood through the sluggish and congested vessels." Nor is it the *materies morbi renalis* alone which would thus affect the heart. Cruveilhier "has more than once observed that chlorotic or anemic palpitations have induced a hypertrophy of the heart, which has survived the cure of the anemia or chlorosis"—thus proving that it had its origin, not merely in deranged nervous action, but in the obstructed condition of the capillary circulation: for "blood [I

quote again from Dr. Basham] with these qualities appears to pass with difficulty through the capillaries, principally on account of the increased number of the colourless cells, which, larger in size than the red corpuscles, exhibit a remarkable tenacity for adhesion to the walls of the vessels." In this patient the kidneys were of extremely small size; there must, therefore, have been considerable obstruction to the passage of the blood into the renal arteries; and when we remember how large their supply is, we can understand how their obstruction would be another cause for increased action in, and therefore hypertrophy of, the left ventricle of the heart. Such cases as the above are to be carefully distinguished from those of organic disease of the heart, in which albuminuria occurs as rather a late symptom.

It remains only to explain the condition of the mitral valve in this case. The situation and persistence of the endocardial murmur during life, combined with the want of proportion between the impulse of the heart and the radial pulse, induced the diagnosis of mitral regurgitation; this want of proportion between the impulse of the heart and the radial pulse is usually ascribed to the impulse of the heart, being in such cases chiefly produced by an hypertrophied right ventricle, and perhaps in narrowing of the mitral orifice it always is so; but in the present instance an hypertrophied left ventricle, sending back a great part of its contents into the left auricle, was mistaken for hypertrophy of the right. The possibility of such an error is recognised by all writers on cardiac pathology since Laennec, who says:—"The *left ventricle* being more prone to thickness, and not less to dilatation than the right, sometimes attains a volume seldom or never acquired by the right; and when its enlargement is enormous, it occupies not only the left precordial region, but extends far under the sternum, where its impulse and sound may be mistaken for those of the right ventricle."

We are familiar with the fact, that regurgitation from the aorta into the left ventricle is perfectly consistent with a healthy condition of the aortic valves. The present case demonstrated the same fact as to the mitral valve. In the former case the valvular insufficiency is caused by a forcible drawing back of the basis of the aortic valves by a widened aorta; in the latter, the abnormal action is also exercised upon the attached extremities of the mitral valve by a widened left ventricle, which has also undergone other important changes. For this addition to cardiac pathology, I believe we are indebted to Dr. Gairdner, now of Glasgow, who



PLATE XII.

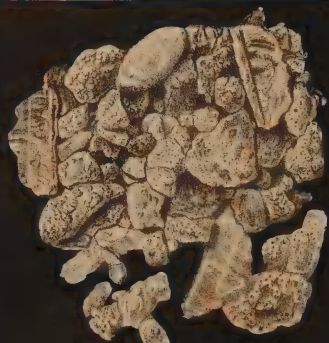


FIG. 1.



FIG. 2.



FIG. 3.



FIG. 4.

demonstrated its mode of occurrence to the physiological section of the British Association in Dublin, in 1857.^a Dr. Gairdner's attention was first drawn to the point by meeting with cases like the present, "in which indications appeared, during the life of the patient, of regurgitation through the mitral orifice, but in which afterwards the valves appeared by no means insufficient." That this condition of the left ventricle is capable of producing a mitral murmur is indubitable; and it will, I think, explain fully the nature of many cases in which the endocardial murmur audible at the apex of the heart has hitherto been considered to be merely functional.

ART. IX.—*Cases of Lithotrity and of Lithotomy, with Remarks.*

By CHRISTOPHER FLEMING, M.D., M.R.I.A.; Surgeon to the Richmond Hospital; Visiting Surgeon to Steevens' Hospital; Lecturer on Clinical Surgery; Member of Council of the Surgical and Pathological Societies; Examiner on Surgery, Royal Coll. Surgeons, Ireland; Foreign Corresponding Member of the Society of Surgeons, Paris, &c.

THE presence of stone in the bladder always excites in the mind of the surgeon more or less of anxiety, not lessened by the consideration of the several expedients which may be required for its cure. That the disease is often to be met with must be admitted, if we place in the category the numerous cases of urinary calculi which spontaneously escape through the urethra, or the many calculi which are being easily removed from or through it; but if we limit our remarks to those hospital cases alone, which demand the operation of lithotrity, or that of lithotomy, the comparative unfrequency of the disease in this city, and, I may add, in the several provincial towns of Ireland, cannot be questioned.^b Such cases

^a See Dublin Hospital Gazette, October 1, 1857.

^b The irregularity with which cases of stone in the bladder appear in our hospitals in Ireland, cannot fail to have attracted attention, and it may not be inapposite to remark, both in reference to the unfrequency and irregularity of their occurrence, that nearly a century ago (1779), Mr. Dease, one of the ablest surgeons and most successful lithotomists which this city, or perhaps any country, ever produced, makes the statement, when writing on the subject of lithotomy, that in so large a city as Dublin, then containing a population of 200,000 souls, there had not been one operation for stone, for nearly two years, and that for four years subsequently there were only three cases cut for stone—and moreover, that the aggregate number of the cases cut in the course of ten years in all the hospitals then in Dublin, was only twenty-eight,

rank amongst the most important to be encountered in operative surgery, and hence a history somewhat in detail, of the progress and results of the respective operations which have been performed, possesses more than ordinary practical interest. The following cases, and the accompanying illustrations, are taken from a series of *Clinical Records of Injuries and Diseases of the Urinary Organs*, now passing through the press,^a and are selected as well on account of the satisfactory results of the treatment adopted, especially in the cases of lithotrity, as of the presence of some peculiarities in their attendant symptoms. An additional value may, perhaps, be attached to them from the fact, that after a lengthened interval, the subject of each case is at present free from any symptoms of his former ailments. The old man who underwent the operation of lithotomy was in hospital some few months back with an attack of acute ophthalmia, and he had then no urinary complaint.

Explanation of Plate.

Figs. 1, 2, and 3.—Portions of fragmentary detritus of natural size and shape, occurring in Cases I., II., III. Small isolated calculus at upper margin of Fig. 1.

Fig. 4.—Six calculi of natural size and shape, removed at single operation of lithotomy—one of the calculi being broken to exhibit its internal structure, and colour.

Chemical Composition of the above.

Analysis by T. W. GRIMSHAW, M.B.

Fig. 1.—Dark portion, oxalate of lime; white portion, ammoniaco-magnesian-phosphate.

Fig. 2.—Oxalate of lime, with a slight outer incrustation of ammoniaco-magnesian-phosphate.

Fig. 3.—Lithic acid, and lithate of ammonia.

Fig. 4.—Lithic acid, and lithate of ammonia.

ten of which cases were successfully operated on by himself. Our statistics on this subject are both incomplete and unsatisfactory. I believe, however, I would exaggerate if I stated that the average *annual* number of cases of stone operated upon throughout Ireland in our several hospitals or infirmaries has exceeded twelve, even since the period alluded to.

^a The delay in the publication of the above work is attributable to causes over which I have not had control. The several plates have now been completed, and many of them have been long since shown at the Pathological Society, at my clinical lectures, and also to many friends. Any claim to originality in their delineations is of comparatively minor importance to me—at the same time it has been deemed only just to myself to attach a special notification to each plate.

CASE I.—*Stone in the Bladder ; Deceptive Sensible Signs of its Presence, and Discovery of, by the Lithotrite ; Characters of the Urine ; Operation of Lithotrity ; Sudden and Violent Hematuria ; Peculiarities and Nature of Detritus ; Permanent Cure.*—A man, aged between fifty and fifty-five years, a carpenter by trade, applied at hospital, with much urinary distress, supposed to be attributable to protracted prostatic disease, the symptoms of which had existed for more than two years. His appearance indicated good bodily health. The irritability of his bladder was very great; the pain he suffered was considerable, and was most intense when the last drops of urine were being discharged. He especially complained of a constant uneasiness in the fore part of the penis, with a scalding sensation in the urethra, which obliged him almost incessantly to squeeze and compress the organ. He stated that there was often a sudden stoppage to the flow of the urine, when his paroxysms of pain became particularly severe, and that they were equally so on any rough movements, in walking or in driving. All pain was limited to the region of the bladder and rectum; there was occasional tenesmus; there was no lumbar uneasiness. The condition of his urine attracted his attention—it was muddy and slimy, and had a heavy disagreeable odour; it was passed at short and irregular intervals, in very small quantities, but reached an average amount daily. He mentioned that he had been repeatedly sounded for stone, and that none could be detected. I admitted him into the hospital in October, 1855.

After rest and suitable treatment, the bladder was examined, and notwithstanding the adoption of every expedient, I failed to find any satisfactory evidence of the presence of stone. There was a solid, dull sensation communicated on moving the sound in the bladder, but not more than could be referred to the condition of the prostate gland, which was felt through the rectum, much enlarged. The character of the urine at this time little varied; it was usually alkaline, or very faintly acid; had a density of 1025 or so; its colour was turbid, its odour fetid, and its deposit viscid and tenacious. Any irritation produced by the examination of the bladder subsided, and the character of the urine, after a few days, improved. I now introduced an ordinary screw flat-bladed lithotrite in place of a sound, and not having felt any sensation of the presence of a stone, I gently opened the blades of the lithotrite, and instantaneously caught a stone measuring on the scale, about one

inch or so.^a The same dull sensation was communicated by the grasp of the stone as at the previous sounding. This examination produced comparatively little irritation; the bladder was afterwards less intolerant of urine; neither was it so of the presence of sound or catheter—on the contrary, the irritability was rather diminished; moreover, the condition of the urine was materially improved, particularly from the daily washing out of the bladder with tepid water; its colour, its reaction and odour, and its deposit, were specially improved. On examining the latter under the microscope, in addition to triple phosphate crystals, usual under such circumstances, numerous minute crystals of oxalate of lime were visible, intermixed with spherical masses of urate of soda, having projecting acicular crystals of uric acid attached to them.^b All these circumstances being taken into consideration, with the size of the calculus, the good bodily health of the man, and his remarkably placid disposition, I decided upon the selection of the operation of lithotrity.

17th October.—This morning (10, a.m.) I proceeded in the ordinary way to the operation of lithotrity on Heurteloup's bed, drawing off the urine in the bladder, injecting it with tepid water to the amount of about six ounces, and then introducing Weiss's screw fenestrated lithotrite—No. 14 in the blades, and about 13 inches the shaft. The instrument was rather large for the urethra, and did not work freely. However, I caught the stone, and had three distinct crushings without removing the lithotrite. There was no perceptible grating sensation communicated during the crushing; but that the stone was broken was evident, not alone from the presence of detritus on the lithotrite, but also from the presence of fragments which escaped through the steel evacuating catheter, and others which afterwards, on its removal, passed immediately with the urine. The man was placed in his bed, between warm blankets, and had a tumbler of hot negus. The amount of suffering experienced was so much greater than I expected, that I regretted I did not use chloroform, and yet I was disposed to attribute much to the large size of the lithotrite, which did not allow of its free sliding movement in the urethra.

^a In the eighteenth volume of this Journal there are some remarks of mine, accompanied with the woodcut of ■ sound and lithometer suited to the child. I have used the same form, of the requisite size and shape, for the adult, with much advantage.

^b Attention to the presence of those spherical masses of urate of ammonia (or soda, ■■ named by some), with acicular uric acid crystals, in certain urinary deposits, such ■■ in the case under consideration, will be useful in the diagnosis of particular forms of calculi.

About an hour after the operation there appeared to be some spasmodic effort to get rid of urine and detritus, but hot fomentations to the hypogastrium and to the perineum, and an anodyne draught, with hyoscyamus, relieved them.

3 o'clock, p.m.—On visiting the hospital I found the man out of bed, passing water, contrary to directions given him to empty his bladder whilst lying horizontally. About half a pint of urine escaped, slightly tinged with blood, and mixed with a good deal of soft detritus, but no particles of any size were visible. He was not in much pain, but yet did suffer from repeated and urgent inclination to pass water. He was directed to have his anodyne draught repeated, and to take throughout the day a demulcent drink, with tincture of hyoscyamus and muriatic acid. Hip-bath at bed-time.

Thursday, 18th, second day after sitting.—Passed the night pretty well, and suffered less pain than usual; paroxysmal attacks, in particular, less frequent; the urine escaped tolerably freely, and contained a good deal of soft powdery material, and also a few solid fragmentary particles—one about a quarter of an inch in length, and less in breadth. Does not feel thirst, and has desire for food. Takes his medicine. To have his anodyne at bed-time.

Friday, 19th, third day after sitting.—Complained much of uneasiness during night, and much scalding in passing water; amount of urine good, and quality better; considerable amount of detritus in deposit. Continue medicine; hip-bath and anodyne at bed-time.

Saturday, 20th, fourth day after first sitting.—Suffered last night from scalding along the urethra, and pain in the region of the prostate gland; no pain or uneasiness of any kind in the perineum on the firmest pressure; urine passed freely, and in fair quantity; much pulverulent detritus, mixed with mucus; no fragments.

Second sitting this day.—Operative proceedings as before; but an ordinary bed, with a firm mattress, and the provision of a rolled pillow under the pelvis, substituted for Heurteloup's bed, and found to be very manageable. Screw fenestrated lithotrite of smaller size introduced with perfect ease; large fragmentary portions separating its blades to nearly half an inch, quickly caught, and three distinct crushings made, with audible grating sound, and without removal of the lithotrite. A considerable quantity of detritus escaped through the evacuating catheter. No fragments. During this sitting, the man was very irritable and restless. A warm drink as before, and an anodyne enema, were directed.

Sunday, 21st, fifth day after first sitting.—Night spent pretty well, yet annoyed with constant burning sensation in the urethra and penis, particularly in the prepuce and glans; uneasiness about rectum; passed a considerable quantity of detritus, with some fragmentary portions; urine in large quantity; much deposit of mucus, with detritus suspended through it. Continue treatment.

Monday, 22nd, sixth day after first sitting.—Appears in better spirits, and expresses himself relieved. Passes water more freely, and in a larger quantity at a time. Only one solid fragment of detritus this day, remainder pulverulent, and mixed with mucous deposit, which latter is less in quantity than heretofore, and devoid of any tinge of blood. Continue treatment.

Tuesday, 23rd, seventh day after first sitting.—Report this day much as yesterday. No detritus of moment in solid portions; darting pains in penis less, also in rectum; quantity of mucus less, but tenacious, and full of pulverulent detritus.

Wednesday, 24th, third sitting.—Eighth day after first sitting. Again proceeded to operation. Chloroform, at his desire administered; effects of it most unsatisfactory; violent automatic movements and complete want of control before anesthesia produced. Steps of operation as at former sittings, and ordinary bed used. Civiale's scoop lithotrite (Charrière's), introduced, and many small fragments of stone crushed with perceptible grating. Detritus-catheter not used, as much of the fluid injected into the bladder escaped during the effects of the chloroform. Warm drink as before; an anodyne at bed-time; hip-bath as usual.

Thursday, 25th, ninth day after first sitting.—Expresses himself as not having suffered from the operation; passed some large and solid portions of detritus, one of which gave great pain, and caused some discharge of blood; in all other respects better; darting pains through penis less, and also uneasiness about the rectum and anus. Mucous deposit in urine as before, portions of it slightly tinged with blood; quantity of urine natural.

Friday, 26th, tenth day after first sitting.—Report favourable; principal painful symptoms subsiding; now can move about with more comfort. Urine acid, and with less deposit; any detritus which escapes is in shape of powder mixed with mucus.

Saturday, 27th, fourth sitting this day.—Eleventh day after first sitting. Chloroform demanded, and exhibited; same unsatisfactory effects. The bladder having been more tolerant of the presence of urine, and now often containing from four to six ounces, I hesitated

to inject any water, or draw off any urine, apprehensive of the effects of the chloroform. I introduced a lithotrite, acting without a screw, by manual pressure, but found the fragment of stone caught so hard and unyielding that I could not crush it. I at once substituted Civiale's scoop lithotrite (Charrière's), and succeeded in crushing, with a sharp and audible click, several fragments. No detritus-catheter was used. Treatment subsequent to operation as before.

Sunday, 28th, twelfth day after first sitting.—Spirits excellent; not the slightest injury from operation. Some complaint of pain in the escape of the larger portions of detritus-fragments, one of which became impacted near the orifice of the urethra, and required force to remove it. Amount of detritus considerable, and consisting of portions of variable size, some very sharp and angular, others thin and scaly; amongst them was one very remarkable, being apparently the nucleus of the original calculus, or a distinct calculus. Urine improved; reaction acid; no tinge of blood in deposit. Treatment as before.

Monday, 29th, thirteenth day after first sitting.—Excellent report; only a few portions of detritus, which were passed with ease; no appreciable amount of viscid mucus or blood; general expression of satisfaction. No impediment to escape of urine, and all pains subsiding. Irritability of bladder less; but as yet passes water every third or fourth hour. Appetite and general health good. Urine greatly improved; reaction distinctly acid; heavy odour removed; average density; mucous deposit less. Bark and nitro-muriatic acid; wine.

Thursday, 30th, fourteenth day after first sitting.—Most favourable report; almost all painful sensations removed, and irritability of bladder so much less that he is now only obliged to pass water twice during the night, and no anodyne is required. Amount of mucus in the urine much less, and less tenacious; no detritus visible in pulverulent form, or in fragmentary portions. He now is able to move freely about the wards without the slightest annoyance.

Wednesday, 31st, fifteenth day after first sitting.—More irritability of bladder last night and this day than for twenty-four hours previously; amount of urine passed at each evacuation between six and eight ounces; and though with deposit of mucus, yet latter not so copious or so adhesive as before. Sensation in neck of bladder as if some portion of the stone yet remained. Walked about a good deal during the day, and without inconvenience. No appearance of fragments, and very little detritus.

Thursday, Nov. 1st, sixteenth day after first sitting.—Report good; irritability of bladder less than yesterday; urine as above. Introduced large evacuating catheter (No. 16), and drew off not less than half a pint of limpid urine. Had sensation of a slight click from a shell of detritus, when last portion of fluid was escaping. Very little mucus or detritus in urine. Took a good deal of exercise during the day. No uneasiness experienced, and can pass his urine in any position.

Friday, 2nd, seventeenth day after first sitting.—Report equally favourable as regards absence of pain and irritability of bladder; quantity of mucus in urine much less, and no detritus worth noting. Sensation of particles of stone in the bladder continues. Civiale's scoop lithotrite (Charrière's), introduced; no detritus discoverable within blades of instrument.

Saturday, 3rd, eighteenth day after first sitting.—Walked about a good deal yesterday without annoyance; this day, spirits very good. Now wishes to leave hospital, as all painful and distressing sensations have disappeared, and he is anxious to resume his work. I examined most carefully for the presence of any portion of detritus, with sound, with lithotrite, and with the large evacuating steel catheter, after injecting the bladder. I even had recourse to Sir Philip Crampton's glass exhausting-apparatus, with his kind assistance, and I failed to discover a trace of any remnant of stone.

Monday, 5th, twentieth day after first sitting.—No appreciable annoyance felt from the above extended examinations. Anxious to return to his employment, and left the hospital this day at his own desire, having promised to report himself occasionally.

Saturday, Jan. 5th.—This morning this man applied as an out-patient, stating that he had been employed at his trade since he left the hospital (now two months past), that since that time, at irregular intervals, he had some irritability of his bladder, and that now, without suffering any previous pain or uneasiness, he had the sensation as if some of his urine had escaped involuntarily, when, on examination, he found his shirt largely stained with blood, and blood flowing rapidly from the urethra. Yet, notwithstanding, he would remain out of hospital under external treatment, being free from suffering, and not wishing to give up his work. In the meantime, the hematuria, being partially but irregularly controlled, ultimately became so severe that he was obliged to apply for re-admission into hospital.

He was re-admitted on the evening of Friday, the 11th January.

Saturday, Jan. 12th.—This morning I inquired carefully into his symptoms, and found that the hematuria was very severe; that the blood escaped, mixed with urine, and that a thick stratum of it lay, coagulated, at the bottom of the urinal. The urine was acid, was of ordinary density, and there was no appreciable amount of mucus in it. The bladder was so tranquil that he was able to retain his urine for six or seven hours, and the stream was never interrupted as heretofore. The amount of bleeding from the urethra was now considerable; it trickled freely from the urethra, and was not influenced by pressure along the track of that canal. I examined carefully for any irregularity, fulness, or tenderness, to indicate the presence of a fragment of detritus. I could detect none; neither was there any indication of such in the prostatic portion of the urethra, as ascertainable through the rectum. The stream of urine was not uniform, and the sensation was that as if the blood in the urethra interrupted it. Rest was enjoined, ice applied to the perineum, and iced drinks were given internally in conjunction with large doses of gallic acid. The urine was carefully examined. The blood appeared to be universally mixed with it, at the same time that the tinge was deeper at the end of micturition; there was no pain, no forcing or straining of the bladder, and there was no mucus of any moment. The density of the urine was about 1020. I could detect no detritus in the deposit. In the absence of retention of urine, or any sensation of obstruction of any moment in the urethra, I did not introduce a catheter. In the course of the day the hemorrhage suddenly increased, and at the same time a sensation was felt as if a portion of calculus was escaping along the urethra; bleeding now was still more violent, the blood highly arterial in colour, and the penis became almost fully erect; an urgent desire to pass water supervened; a sudden interruption to the stream took place, when a large shell of stone was found impacted near the orifice of the urethra. It was with some difficulty removed; it was very sharp at its edges and angles, and measured in its longest diameter at least a quarter of an inch. The first pain experienced since the commencement of the hemorrhage was now, when this fragmentary detritus-portion blocked up the canal of the urethra; and on its removal the hemorrhage and all other symptoms ceased, and after a few days the man would leave the hospital.

About a month afterwards he called at the hospital, stating that

he was perfectly well, and I have from year to year repeatedly seen him since, perfectly free from any urinary complaint.

Fig. 1 in the plate exhibits, of their full size and shape (many conchoidal), several fragmentary portions of detritus collected in this case. The distinct calculus alluded to is also visible, and it is questionable whether this is to be considered as the nucleus of the original calculus, or whether it may not have been a separate calculus altogether. The weight of the fragmentary detritus collected, when dried, was nearly two drachms. The remarkable want of correspondence between the rational and the physical signs of stone in the bladder, as tested by ordinary sounding, is most important to bear in mind, as errors in diagnosis may otherwise occur. It appears to be satisfactorily accounted for by the amount of viscid mucus present in the urine, as a calculus enveloped in such coating (if it even were a mulberry one) will not communicate to any metallic instrument the sharp click of a calculus situated in non-mucous urine. I have known the same deceptive sensation produced by coagulated blood around a stone, and this, more than once, deceived in the operation of lithotomy, when much hemorrhage had taken place into the bladder, and much annoyance was hence experienced, in what I may at present term the forceps, or last trying stage of that operation. The stone will be within the range of the forceps, and perhaps within its grasp, and the surgeon will not feel it under such conditions. The knowledge of such contingency is hence obviously of much practical importance.

I may here allude to the case of a little boy in hospital at this period, with stone in the bladder, accompanied with unequivocal signs, both rational and sensible, upon whom I performed the operation of lithotrity. I had procured a child's *brise-pierre* from M. Charrière, of Paris, and first used it as a lithometer and sound; at the blades it corresponded with No. 8 on the ordinary scale, in the shaft it was not more than No. 6. I ventured to crush the stone, which was not larger than a garden pea. The force required was considerable. The sound elicited was sharp and loud. The child, although fully under chloroform, was by no means manageable, as repeated straining efforts of the bladder were being made, exactly as in a fit of the stone. Some detritus was removed in the lithotrite, but evidence of any subsequently was most unsatisfactory, from the difficulty of collecting it; and I may remark that the lithotrite was a blade-lithotrite, *unfenestrated*; indeed, I have not seen any *fenestrated* lithotrite suited to children. My impression respecting

lithotritry was so unfavourable that I have not since adapted it in boys. The urine in this case, and the detritus, indicated the lithic calculus, the most frequent in the child. This child, now aged ten years, lives in the neighbourhood of the Richmond Hospital, and has never since had a symptom of urinary complaint.

CASE II.—*Stone in the Bladder ; Rational and Physical Signs ; Characters of the Urine ; Oxalate of Lime Calculus ; Operation of Lithotritry ; Peculiarities of Detritus Fragments ; Permanent Cure.*—A young man, aged twenty years, was sent to me from the County Meath, labouring under marked symptoms of stone in the bladder, from which he had suffered for very many years. He was of medium frame and stature, and his general health was excellent. Now his desire to pass water was hurried and frequent, and seized him in irregular paroxysms, which were brought on by any rough movements. He did not observe that the stream of urine was at any time suddenly interrupted during micturition ; neither did he, at the end of it suffer any additional pains. His penis was enormously developed, and was almost persistently in a state of semi-erection. He never had hematuria, and any change in the appearance of the urine had not attracted his attention. Such was his condition on his admission into Richmond hospital in April, 1861.

There was no difficulty in detecting the presence of a stone in the bladder, and from the sharp ringing sound elicited during the examination, it was concluded that the stone was principally oxalate of lime. The character of the urine tended to confirm this opinion. Its colour was a light straw, its reaction acid, its density about 1020, its odour not disagreeable, its deposit was that of a peculiar semi-transparent tomentous mass^a which I almost always find to indicate the presence of oxalate of lime, and there was no appreciable amount of mucus.

The stone was caught easily with the lithotrite, and found to measure about one inch, but any extreme accuracy regarding its size was not attempted, the man being very irritable, and his bladder and urethra being equally so. The question of lithotomy could not

^a This peculiar deposit is accurately outlined in one of my plates. When it is once carefully studied, it will be found to be almost conclusive as to the presence of crystals of oxalate of lime. Indeed, if such specimens be examined, in the recent state, even with the naked eye, under a strong sunlight, minute scintillating points will be visible through the deposit, and these, under the microscope, will be found to be oxalate of lime crystals.

be entertained, as the man was determined not to submit to any cutting operation, and indeed, from the size of the penis, I was not an advocate for it. Lithotrity was hence decided upon, and after some preparatory treatment I proceeded to accomplish it.

Tuesday, 7th May, 10 o'clock, a.m.—This day the first sitting took place. I did not use Heurteloup's rectangular bed; I found a common hospital bed, prepared as in the former case, to answer all requisite purposes. The man was very timid, and apprehensive of pain. However, I did not give him chloroform. Before my visit to hospital, according to his statement, he had passed about four ounces of urine, so that I was satisfied with injecting about the same quantity of tepid water into the bladder. I now introduced with perfect ease, and with trifling pain, Weiss' improved fenestrated lithotrite, of No. 13 size. Pressing the instrument gently towards the back part of the bladder, and with equal gentleness raising the handle, whilst I kept the shaft steadily fixed, I lifted the movable blade of the lithotrite to the distance marked in the measurement above noted, when, approximating it towards the fixed blade, I at once felt that I had caught the stone. Getting a firm grasp of it, and drawing the lithotrite a short distance forwards, I commenced the screw movement. During its working, which required much force, an irregular grating sound was audible, when a loud crack followed, which alarmed me as to the safety of the blades of the lithotrite. It is no exaggeration to state that this sound was distinctly heard at a considerable distance in the operating theatre. The sound was an abrupt sound, unaccompanied by any crushing sensation in the closing of the blades. Again the blades were separated, and during a second screw movement the same result followed. I ceased from any further manipulations, and removed the instrument. In about ten minutes afterwards he passed nearly half a pint of urine in an uninterrupted stream, in the erect posture, and with tolerable ease; there was not visible the slightest tinge of blood. He was ordered a draught with tincture of hyoscyamus, a tumbler of hot wine negus, and a hip-bath.

Four o'clock, p.m.—I visited the hospital, and found this man comparatively free from any complaint, and extremely difficult to be kept confined to his bed. He had passed water once in large quantity, and without much uneasiness, but with the sensation that the stone was broken, and that the broken pieces were grating against each other. No detritus had passed. Demulcent drinks; hyoscyamus draught and hip-bath at bed-time.

Wednesday, 8th May, day after first sitting.—This morning the report was not very good; he had not a hip-bath as directed; he suffered a good deal through the night, particularly at the end of micturition; he had no rigor, but the pulse was quick, and he had some thirst. The urine passed in the glass urinal was acid, muddyish in colour, and threw down an opaque deposit, in which very many minute particles of detritus, of a dark colour, were visible. Saline draughts, with tincture of hyoscyamus, were directed, and a hip-bath at bed-time.

Thursday, 9th, second day after first sitting.—Better night; benefit from hip-bath, but yet much pain at the end of micturition; no feeling of obstruction in urethra; pain referred to about the root of the penis; passed water more frequently than usual; urine much as noted; no detritus of any moment.

Wednesday, 15th, eighth day after first sitting.—Two days back, at his desire, chloroform was administered, and the effects were so unsatisfactory that all operative interference was necessarily suspended. The vesical irritation produced was considerable for many days; calls to make water were more frequent both night and day, and pain was always referred to the perineum, immediately in front of the anus. No fever could be said to be present, but there was much irritability and sensitiveness. Under local and general treatment these symptoms subsided, and now a second sitting was decided upon.

Second sitting.—He was removed to the operating theatre, and placed in proper position, under the influence of chloroform. Previous to its full anesthetic effects I proceeded to inject the bladder with tepid water, and although doing so with the utmost gentleness, so violent a vesical paroxysm was produced that the urine and fluid injected were forced out along the side of the catheter. I withdrew it when under full anesthesia. Reinjecting the bladder I proceeded with the operation, using Weiss' fenestrated lithotrite, No. 14 in size at the blade, less in the shaft. It entered the bladder freely. Portions of stone were easily and quickly seized, and crushed with varied force—some appearing to fly from under the lithotrite, whilst others were audibly cracked. All this was effected in a short space of time, and without the escape of any urine during the sitting. The man awoke out of his sleep, had an anodyne draught and some sherry negus, and was placed in bed. The urine was not drawn off, neither was he urged to pass it after the operation.

4 o'clock, p.m.—Visited the hospital, and found that a considerable portion of detritus had escaped; particles distinct, angular, and of different sizes, presenting, on examination of the different surfaces, evidences of the ordinary mulberry calculus. Some fragments are accurately outlined in the plate in Fig. 2, nodulated externally, in apparently aggregated masses of globular form, and in size about that of an ordinary garden pea, all as if cemented together and covered by snow-white glistening particles of oxalate of lime and triple phosphate. On the outer surface of many of these fragments there was a beautiful appearance somewhat resembling pearl-spar crystals, in some parts being as if transparent, in others opaque, the former being more particularly visible in strong sunlight. The section of many of those fragments presented a laminated and undulating appearance. Hip-bath; continue anodyne.

Thursday, 16th, ninth day after first sitting.—This morning good account, passed fair night; scalding and pain in passing detritus, especially some large fragmentary pieces; in other respects tolerably easy; no pain in or about region of bladder; sensation uncomfortable in perineum as before, and distinct sensation of obstruction from fragments in urethra; will not permit any interference to remove them.

Friday, 17th, tenth day after first sitting.—Report cheering. Very large portions of detritus, one sharp and angular, and so large as to pass with difficulty through No. 16 on the guage scale; it escaped by itself after repeated efforts to dislodge it, and was followed by some hemorrhage; its several edges were sharp, irregular, and laminated, and evidently consisted of an oblique chip, including much of the outer surface of the calculus. General condition comfortable; sensation that no more of calculus remained; at the same time there was pain in micturition, and especially at latter end. Examined specimen of urine passed with the portion of detritus—colour, brownish; odour, natural; density, 1018·20; reaction, acid; deposit copious, opaque, brownish-yellow, slow in forming, with supernatant fluid clear. Under the microscope, blood corpuscles and pus corpuscles were distinctly visible, also some crystalline colourless masses, with a few octahedra of oxalate of lime.

4 o'clock, p.m.—Comparatively free from pain, but yet some at end of micturition; passed two large coagula of blood, darkish and clotty; no detritus traceable. Demulcent drinks, with nitro-muriatic acid; hip-bath if required.

Wednesday, 18th, eleventh day after first sitting.—Tolerably

quiet, but yet no pain after micturition; urine dark chocolate-colour throughout, with some flakes of coagula floating through it; copious deposit; acid reaction; no detritus of moment; if any, pulverulent. Continue medicine.

Thursday, 19th, twelfth day after first sitting.—Report favourable; good night; pain in micturition less; blood disappeared; disposed to get up and walk about; condition of urine much as yesterday, with very slight trace of blood in deposit; no detritus of moment.

Friday, 20th, thirteenth day after first sitting.—No special complaint; no return of hematuria; some more frequency in micturition, and more pain at end of it; attributed to walking too much; no detritus of moment. Continue acid, and bath at bed-time.

About this time he became very irritable and unmanageable, and would not submit to any additional sitting unless under the influence of chloroform. Its effects were decidedly injurious, and increased intensely all his urinary suffering so much, that any interference in way of operation was wholly inadmissible. Ultimately, however, the symptoms became less severe; at irregular intervals different portions of detritus escaped in the urine, and it assumed a more healthy character; there was much less mucus in its deposit, and there was no appearance of blood. He now became dissatisfied, and wished to leave the hospital, when I persuaded him to remain, and again submit to treatment.

Thursday, June 6th, fourth sitting, 10 o'clock, a.m., thirtieth day after first sitting, and sixteenth day after third sitting.—Operation in ordinary bed as before. Chloroform necessarily exhibited; full anesthetic effects produced. Bladder not injected, urine having been retained for some hours. Flat-bladed lithotrite, No. 14, 15, introduced; many fragments easily caught and crushed; pulverized as much as could be. Warm negus and anodyne draught given before effects of chloroform fully disappeared. Directions to remain in bed, and not to pass water until inclination to do so.

4 o'clock, p.m.—Called at hospital. Saw urine passed an hour after sitting; quantity about six ounces, and not a trace of blood in supernatant fluid or in deposit; much detritus, some in large masses about size of small pea, escaped without pain or much of forcing. Demulcent drinks; to have anodyne and bath at bed-time if required.

Friday, 7th.—Most satisfactory report; detritus escaping freely, and in less quantity; sensation as if all portions of stone had now been removed; some tenderness and scalding along the track of the

urethra, and after micturition; all other symptoms improved. Treatment as before; add to demulcent drinks nitro-muriatic acid.

12th June, thirty-five days after first sitting, sixth after fourth sitting.—Such decided improvement in all symptoms that he determined to leave the hospital this day. No detritus during the last two days; no irritability of bladder; no uneasiness of any moment; note of urine satisfactory. Left hospital.

August.—Letter from, stating that he was quite well.

May, 1863.—Called at hospital; remains free from any symptom of his disease; in rude health.

In Fig. 2 in the plate, portions of the detritus which escaped spontaneously through the urethra in this case are very truthfully delineated. Their size, and their peculiarly angular form, are by no means exaggerated, they are most accurate. Their outer, irregular mulberry surfaces, can also be recognized, with the frosty coating mentioned in the details given; and the laminated appearances of the lateral sections will be also visible to some with the naked eye, to others with a lens. The weight of the detritus, collected and dried, was within a few grains of three drachms. The great density and weight of those several fragments are very remarkable.

CASE III.—Irritable bladder, with Recurring Attacks of Retention of Urine; Supervention of Rational Signs of Stone in the Bladder; Sensible Signs; Conclusive Character of the Urine; Operation of Lithotrity; Nature of the Detritus; Cure, with Recovery of the Functions of the Bladder.—A man, a mason by trade, and aged between sixty-five and seventy years, was sent to me from the County Cavan, with symptoms of urinary disease, which distressed him for more than two years. The irritability of his bladder was very great at the commencement, and ultimately terminated in occasional attacks of retention of urine, occurring at irregular intervals. He was now admitted into one of our county infirmaries, and whilst there was taught to relieve himself from retention of urine by the introduction of a catheter when required; upon these symptoms, latterly, others very painful supervened. He often had incontinence of urine, as well as retention, and the catheter often failed to give him relief as before; he required its use more frequently; during its introduction, particularly as it reached the bladder, his pains were excessive, and still after the urine was drawn off he suffered acutely. His urine, moreover, became muddy

in colour and heavy in smell. He applied at hospital with a recommendation from his medical attendant, and I admitted him in May, 1863.

His journey to town increased in intensity all his symptoms. His urine as passed at that time was clouded, feebly acid, in density about 1018, and it had a fetid odour. Its deposit, on resting, was opaque; contained urates in large abundance; its supernatant fluid was clear, and there was no albumen. Under the microscope, granular bead-crystals of urate of ammonia were visible in the deposit, intermixed with some tabular plates of uric acid crystals, and some mucous corpuscles, apparently pus corpuscles in size and outline.

The bladder was then tolerant of the presence of urine; the man was in fair bodily health for his age; he was apparently a placid person; and the prostate gland was not more enlarged than to be expected at his period of life.

Satisfied with the presence of stone in the bladder, from the rational signs detailed by the man, expecting its physical signs from the account of his medical attendant, and concluding the nature of the calculus present from the character of the urine, I introduced as a sound, and as a lithometer, a fenestrated screw lithotrite of ordinary size (No. 14), intending merely to measure the stone if found, and purposed to defer the crushing process until a future day. I at once caught the stone in the ordinary horizontal position of a patient lying in his bed. The sound from it was an audible sharp sound; it measured an inch or so, and out of the grasp of the lithotrite I could not shake or loosen it by any manœuvre. I had no alternative—I worked the screw at once, and crushed the stone. Many distinct crushings of the broken pieces were accomplished at the same moment, without any difficulty, when I removed the lithotrite. I had not injected any fluid into the bladder, neither did I now use the evacuating catheter. The bladder bore the several manipulations well, and voluntarily discharged itself about twenty minutes afterwards without much uneasiness, the urine being mixed with much pulverulent detritus. Any irritation produced was slight, and was easily controlled by mild anodyne treatment.

The daily records taken in this case would be only tedious to recapitulate from their great similarity to those already noted; the several sittings required, and the intervals between them were nearly the same; the effects, as shown by the deposits, both

fragmentary and pulverulent, were much the same, and their final results, though occasionally interrupted, were equally satisfactory. The man was able to empty his bladder without the necessity of a catheter, he was free from any painful sensation about the bladder, and the urine had gradually acquired a healthy condition. The last portion of fragmentary detritus was rather tedious in being removed. It was, however, ultimately and most satisfactorily so, and on this occasion the bladder was injected as largely as it could tolerate, and certainly to the extent of containing at least eight ounces of tepid water. This amount of fluid was thrown in with a gum elastic bag of requisite size, provided with a conical shaped ivory tube, which accurately filled a catheter (No. 8), almost straight, and with small openings, so that the fluid could necessarily only enter the bladder very slowly and in very minute quantities at a time. Civiale's scoop lithotrite (Charrière's), with the fixed blade hollowed or spoon-shaped, and the movable short and abrupt, was now introduced, and struck the particle of stone, when, by a sharp and sudden turn of the disk, the particle was caught and pulverized. After this sitting all symptoms of uneasiness quickly subsided, and the man left the hospital in about a week afterwards, in the beginning of August, free from his former sufferings, and able to empty his bladder without the necessity of a catheter. His urine was not as free from mucus as might be wished, but yet was from day to day improving in every respect. I had a letter from him two months subsequently, stating his comfortable condition and freedom from his former symptoms, and I have lately heard from his neighbours that he continues well.

In Fig. 3 an outline is given of the appearances of the portions of fragmentary detritus collected. Their size and resemblance in shape to those in Fig. 1 will attract attention, and also their chemical composition. The weight of all, when dried, was about two drachms.

The necessity for reporting the daily progress of the above cases may be questioned by some, and yet it is very desirable that the young surgeon, or the advanced student (for whom alone these records are intended), should be acquainted with the several contingencies likely to arise in the progress of any operation he may contemplate. The operation of lithotritry is of rare occurrence in our hospitals; and as they are often the most reliable, and, if necessary, the more legitimate places for clinical investigation, it is desirable to place the results observed on record. Many useful reflections must

suggest themselves in considering the history and progress of these cases, both previous and subsequent to their treatment; and it does so accidentally happen that rarely, indeed, could a series of cases be adduced more instructive and more demonstrative of the great value of the operation of lithotrity, even under the very unpromising circumstance of advanced age, and of nature of calculus, apart from other embarrassments, rendering extremely questionable a favourable issue. Successful as was the result of these cases, it is possible that the cure might have been more rapidly effected, and with perhaps less of suffering on the part of the patient, under the present advantages of more modern lithotrity. Now, the bed of Heurteloup is considered obsolete; the injection of the bladder before operation, or its being emptied after operation, conditional; the position of the patient in some degree optional, and that of the surgeon in respect to his patient often equally so; the selection of lithotrite, and mode of introduction, optional or conditional; the manner of seizing or crushing the stone, or the fragment of stone, shall I say, fanciful; the removal of the detritus, fragmentary or otherwise, spontaneous as regards the bladder, or mechanical, slow or abrupt, as effected by the surgeon; and ultimately the number and repetition of the lithotrities conditional. Such is a hurried summary of the particulars of the several steps of the operation of lithotrity as usually performed. The operation now is very rapidly accomplished, and it is not surprising that it should be so, when we consider the improvements latterly made in the mechanism of the several appliances required. In none have these improvements been more manifest than in the light and beautiful lithotrites of Weiss, so commonly used at present. Whether fenestrated or not, the improvement made in them, both as regards size and details of construction, are incomparable, they facilitate and expedite the uses of those instruments, and admit of their more general introduction from their varied size. They possess the great advantage of retaining the stone or fragment of stone between the blades, during the moment of changing the sliding into the screw movements, and this without the necessity of any alteration in the position of the hand of the operator, from a provision whereby a slight movement of the thumb accomplishes the object. Moreover, with their fluted and cylindrical handles (suggested by Mr. Thompson) they also possess the great advantages by which, with a slight movement of the finger, the most delicate turning of the blades laterally, or even to complete rotation if requisite, may be accomplished; and moreover,

a firm and immovable grasp of the instrument taken, and thus the crushing of the hardest stone be as safely as certainly secured. Other advantages derivable from the special form and action of the blades of those valuable instruments must attract attention when investigated. It would be out of place here to enter into the consideration of them. With the instrument in hand they can be carefully studied, when the influence of the slightest elevation or depression of the handles of the lithotrite, or deviation from the proper direction of the shaft, will show the necessity for continued attention on the part of the operator to those minute particulars, as indicating the position of the blade, whether closed or open, and directing their safe and successful application. A few passing remarks may be permitted, in conclusion, respecting the position of the patient and that of the operator, the selection of the lithotrite and its mode of introduction, and its manipulation in the bladder, and the removal of the detritus. I prefer that the patient should be placed opposite me, either on a table of requisite height, with or without provision for the elevation of the pelvis, or, as I often find very convenient, at the foot of his bed, arranged as may be considered best. In the selection of the lithotrite, as to number I am guided by the calibre of the urethra, always providing that the sliding portion of the shaft shall have free space for loose movements, and I select either the lithotrite fenestrated or with entire blades, according to the special object in view.

If, as a lithometer to measure a stone, the blades should be plain—equally so, but shaped according to the objects in view, if for crushing or pulverising fragments; but if at a first sitting and to break a stone, the lithotrite must be fenestrated. During its introduction I latterly always place myself between the thighs of my patient, taking care that they shall be widely separated, and firmly supported, with the legs and feet properly placed. After some trials I find this position preferable to any other. Doubtless, the lithotrite may be introduced, when used as a lithometer, as the ordinary catheter, but for lithotritry purposes I prefer the position I mention. I always take care to have the abdomen fairly exposed, so that the umbilicus may be held constantly in view as an unerring guide to the safe direction of the instrument. I pass it with my right hand into the urethra, and press it onwards towards the perineum, at the same time drawing forwards the penis to the extent required. I now having reached the perineum, transfer the lithotrite to my left hand, when, gently depressing its handle, or

even allowing it to fall with support by its own weight, the bladder is immediately reached. In this latter movement I fancy I have often found it a good plan to divert the mind of the patient from this—the most important, and often the most painful movement in the operation—by making gentle pressure with the palm of the right hand over the pubic region, or with the ends of the fingers over the perineum in the seat of the angle of the lithotrite, and so to assist its onward passage. The amount of depression of the handle of the instrument above alluded to must be measured by the presence or absence of prostatic disease, or by that of the bar-like ridge at the neck of the bladder. In either case the depression required may be considerable, and in both a certain amount of additional force is necessary to secure the satisfactory entrance of the instrument into the bladder. The depth to which it should be passed will much depend on the same contingencies. The subsequent manipulation must be conducted with the greatest caution and gentleness. By some surgeons the stone will be seized instantaneously, and in any position of the patient; whilst by others, select what position they may, the greatest difficulties and perplexities will arise, and very often there is a complete failure in catching the stone. I have noted in the history of the cases detailed, that it is well to open the lithotrite whilst engaged in the operation to the extent to which the movable blade had reached on the scale when the stone was recognized. If this be attended to it will prevent the partial separations of the blade which are as useless as they are often most injurious.

Those directions for seizing and crushing the stone—which are now fully detailed and discussed in all the recent works on surgery, and in none more satisfactorily and clearly than in the article on “Lithotritry,” in the fourth volume of Holmes’ *System of Surgery*—I cannot now further consider. It seems some improvement was called for in the detritus or evacuating catheter, when the use of such might be advisable, and I give a woodcut, executed by Mr. Oldham, of one which I have found useful (Figs. 1, 2, and 3). Its size may vary, but to be efficient it should not be under 14 or 16 on the scale. Its curve is short and abrupt, and one large opening exists on its concave surface, near its extreme end. There is a strong stilette, with a ring at one end and a chain attached to a firm pellet at the other; this stilette and chain fit the catheter loosely, and its length exceeds that of the latter by an inch or so. The advantages of this instrument I have found to be, that

Fig. 1.

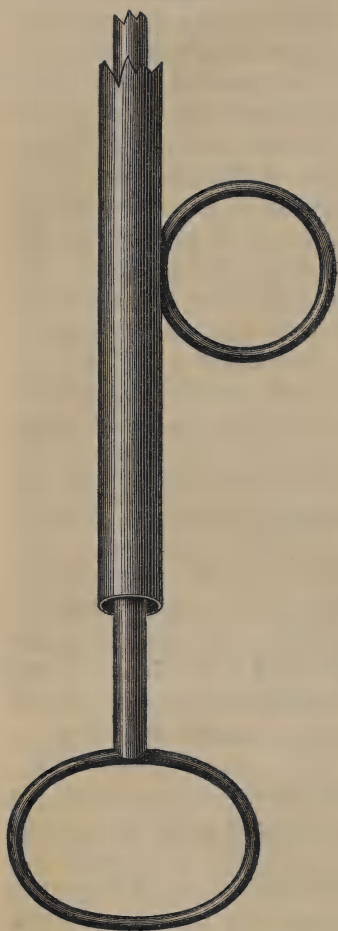


Fig. 2.



its short curve admits of its extremity being turned safely towards the ordinary situation of detritus in the bladder, and hence that it may be more satisfactorily commanded and influenced by injection thrown in. The provision in the stilette will admit of its pulverizing or clearing detritus without at the same time interfering with the spontaneous escape of urine or other fluid from the bladder. To this catheter may be attached the several varieties of syringes in use, and amongst them that which has been recently recommended, and which may be termed a "flushing syringe." I have had a wood-cut executed of an addition to this syringe (Fig. 4), which

Fig. 3.

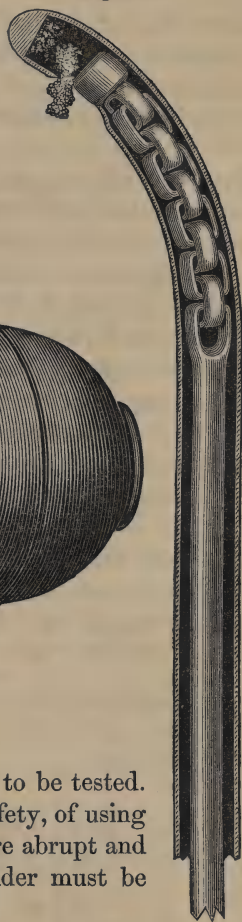
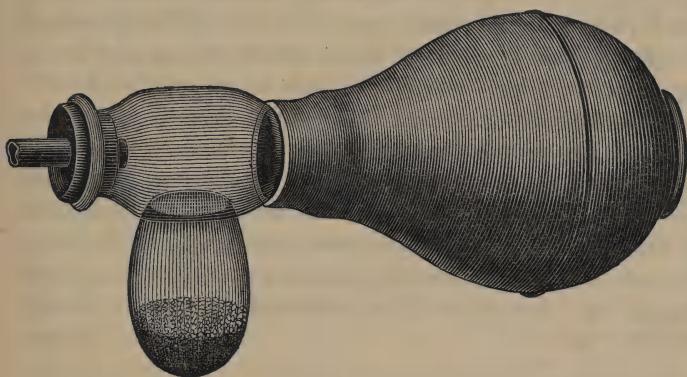


Fig. 4.



may, perhaps, be found efficient, but it is yet to be tested. I doubt very much the efficiency, if not the safety, of using such class of syringe in urinary diseases, where abrupt and sudden changes in the capacity of the bladder must be produced.

The history of the case of lithotomy identified with Fig. 4 in the Plate is, of necessity, deferred. Suffice it to say, the operation was the lateral operation—the several calculi delineated were removed within a few seconds, and the recovery of the man has been perfect. His age was beyond 60. He was admitted into the Richmond Hospital in September, 1859, and left, free from all urinary suffering, in the early part of the November following. He has had neither retention or incontinence of urine since; and he has on many occasions presented himself at the hospital free from his former complaint.

ART. X.—*A few Remarks on Sun-stroke, or Heat Apoplexy, with Four Detailed Cases, and their Treatment.* By C. P. BAXTER, A.B., M.B., L.R.C.S.I., Assistant-Surgeon 93rd Sutherland Highlanders.

THE following cases of sun-stroke, or heat apoplexy, occurred amongst the men of the 93rd Highlanders during the hot seasons of 1864 and 1865, while the regiment was quartered at Sealkote, a station situated in the Rechna Doab division of the Punjab, on the left bank of the Chenab river, about sixty-three miles N.N.E. of Lahore, and in lat. $32^{\circ} 29'$, and long. $74^{\circ} 33'$. The district of Sealkote extends over an area of 1,457 square miles, and has a native population of about 641,782. It is a wide plain of sedimentary deposit, perfectly open, and almost completely under cultivation, and free, to a certain extent, from malarial poison. The neighbourhood of the station is planted with trees, and vegetation is luxuriant in it. The rains do not come on here with the same regularity they do in the lower portions of Bengal, and the thermometer at certain times rises very high, as will be seen from the accompanying table, taken from observations made regularly throughout the year:—

Date	Max.	Med.	Min.	Wind	Date	Max.	Med.	Min.	Wind
1864					1865				
24th June,	106	95	85	W.	24th June,	108	98	88	S.E.
25th „	106	95	85	W.	25th „	106	98	90	S.E.
26th „	109	98	86	E.	26th „	106	99	91	S.E.
27th „	110	98	87	E.	27th „	107	99	92	S.E.
28th „	111	98	88	E.	28th „	107	99	92	W.
29th „	110	98	87	E.	29th „	110	101	92	W.
30th „	109	99	90	E.	30th „	106	100	95	W.

It is not, however, in my opinion, when the greatest heat is registered by the thermometer, nor when the sun's rays are most intense, that men are stricken down by this fearful disease; neither do I think that sun-stroke is frequently caused by the direct action

of the sun's rays striking the body, unless it be exposed to them for a long time—or in a quiescent state, as in the case of a man getting drunk, falling down on the plain, and lying there for some time, with the sun beating on him; such a man is nearly sure to get sun-stroke, and die from it also. The state of the atmosphere I consider most conducive to sun-stroke, from the experience I have had, is, when it is largely impregnated with watery vapour, with the thermometer ranging from 95° to 102° in the shade; when clouds hang over the head, when there is hardly a breath of wind, and the day is what is called a heavy one at home. The body then, if in a healthy condition, sweats very profusely, and this, I think, is one of the outlets that keeps one in health, and partially assists in preventing congestion of the internal organs. For in sun-stroke, as will be seen in the detailed history of the cases, the skin never perspires, remains perfectly dry, and even during recovery it is sometimes found difficult to keep up its action. These facts, as to the state of the atmosphere and the action of skin, will all better appear on reference to the cases.

This thermometrical table gives the temperature for the weeks during which these cases occurred in two succeeding years, and shows that these weeks were by no means the hottest in the season:

Date	Max.	Med.	Min.	Wind	Date	Max.	Med.	Min.	Wind
1864					1865				
15th July,	99	91	83	E.	8th July,	98	91	84	E.
16th „	100	93	85	E.	9th „	93	89	84	S.
17th „	103	95	87	S.E.	10th „	99	93	86	E.
18th „	98	95	92	E.	11th „	99	93	86	E.
19th „	94	89	83	E.	12th „	99	93	86	E.
20th „	88	87	85	E.	13th „	94	88	82	N.E.
21st „	99	92	85	N.W.	14th „	87	82	76	W.

I have already given a table showing the periods during which the thermometer reached its greatest height, and during which, also, sun-stroke was not prevalent.

I will not here enter into the pathology of the disease, as I know there are various opinions on it, but will merely state that it

appears to me, in most cases, to be caused by the body absorbing heat through the blood, which retains it up to a certain stage, when it becomes too hot for the organs it has to come in contact with, and then acts as a paralyzing agent on the cerebro-spinal system or heart, or sometimes partially on both, thus forming the three kinds of sun-stroke mentioned in books, which are much more easily described than discriminated at the bed-side.

Of the pathology of the disease I cannot, from these four cases, give much information, as death only occurred in one of them; any abnormal condition of the important organs I could notice on making the *post mortem* I have faithfully given.

On considering the *post mortem* appearances I have seen in cases of cholera, Peshawur remittent fever, and sun-stroke in this country, they all seem to me to have a close analogy to one another.

The treatment followed by me in these cases was as follows:—When the patient was carried to hospital in an insensible state, with the peculiar dry, burning skin of the disease, and with laboured breathing, the cold douche was the first thing resorted to, and this was given in the following way. Four bheesties, or water carriers, were set to work to fill their mussacks or pig-skins; they then came on, one by one, and while the patient lay naked on a low cot, they mounted on a high stool, and let the water pass in a full stream over his head, neck, spine, and body generally. The next thing done was, a strong stimulating enema was given, and sinapisms were placed over the heart if it was weak, and to the abdomen if required. As soon as the patient could swallow, carb. ammoniæ was given in five grain doses. The cold water was continued over the body from a height, frequent observation as to the action of the heart, and the state of the pulse, being made. After a short time the great heat leaves the body, but continues in the head, which, however, in some cases, rapidly becomes cool. If the water is then discontinued, it is quite marvellous the rapidity with which the burning heat returns, and with it the douche must be resumed. In two of the following cases successfully treated, I have made myself certain by measurement that not less than 200 gallons of water were poured over the patient before it was safe to stop it. During the recovery an occasional blister to the scalp, or behind the ear, proves useful if any head symptoms supervene. As will be seen in the cases, I have continued ammonia for a number of days. I also gave quinine to act as a tonic, and supported my patients with light and nutritious diet.

I.—*A Case of Sun-stroke, with Complete Coma; Treatment, Death, and Post-mortem Appearances.*—Private Robert B—, age thirty-eight, a well-conducted, sober man, was admitted into hospital at 7, p.m., on the 17th July, 1864, for the above disease. I saw him a quarter of an hour after admission; his condition then was as follows:—He was perfectly insensible; breathing very laboured, and moaning occasionally; pupils quite insensible to light or darkness; skin perfectly dry, and a burning heat all over the body; heart's action tumultuous; the two sounds not distinct; pulse small and compressible; general irritation of the muscular system. When I arrived, I found his clothes had all been taken off, and he was on a low cot, having a stream of cold water poured from a height over his body. A stimulating and purgative enema was immediately given to act on his bowels, and ten grains of calomel, with two drops of croton oil, were placed on his tongue, as soon as he showed any power of swallowing. The enema soon acted on his bowels; and after the water had been continued for an hour the great heat of skin subsided slightly, and he seemed a little roused. The water was then discontinued, and a draught containing five grains of carbonate of ammonia, in an ounce of camphor mixture, was given. Very soon, however, the burning heat of skin returned, and with it, the conjunctivæ were observed to become much congested or injected, and the pupils dilated.

The scalp was now shaved, and strong vesicating collodion applied. The cold douche was again resumed to the body, and continued steadily. After a little while he began to give long and sorrowful moans, which succeeded each other at regular intervals. He had two convulsive fits; and although the cold water was kept constantly playing on him, the burning heat of skin still continued. The moaning became longer and weaker; the pulse and heart's action got weak. About 2, a.m., on the 18th, he had a slight fit, and twenty minutes afterwards he died. Up to the moment of his death, and for long after it, the burning heat of skin remained. The history I have been able to get of this man from his comrade is:—On the 17th he was at his duty at 12 o'clock (it was indoor work); about 2 he complained of not feeling well, and lay down in his barrack room, on bed. About 3.30 he got up, smoked a pipe, and talked to the man next him, and about 4 lay down again, and fell asleep. At 6, p.m., his comrade noticed he was sleeping heavily, tried to rouse him, but found it impossible; he then sent for the barrack doolie, and had him carried off to hospital, a distance of not

more than about 300 yards, and his symptoms on arrival there I have already given.

From this case it will be seen he was not a man habituated to intoxication; he was not exposed throughout the day to the sun's rays, as he spent all of it in a house; and on referring to the thermometrical table, the temperature by the thermometer did not nearly come up to some of the other days we had through the hot season of 1864, but the peculiar moist, depressing state of the atmosphere before mentioned prevailed.

Post mortem six hours after death.—Body generally well developed; marks of ecchymosis on the chest, arms, and back.

Head.—On removing the calvarium, the blood vessels of the brain were found congested, and on making a section of the brain numerous congested spots were seen in it. The ventricles contained no fluid. There was no abnormal quantity of sub-arachnoid fluid.

Chest.—The lungs were crepitant throughout, both of them congested at their bases. Heart normal.

Abdomen.—There was slight congested appearance of the omentum, and of the hollow viscera generally. The stomach was much distended with air. Spleen a bright blue colour, and small. Liver healthy. Kidneys a good deal congested. No other *post mortem* appearance of any importance.

II.—*Case of Sun-stroke, followed by Partial Paralysis; Treatment and Recovery.*

Francis B—, age thirty-seven, a tall, delicately-built man of temperate habits, was admitted into hospital for the above disease, on July 11th, 1865, at 6.30, p.m. The history of his case is:—On that morning he had been discharged from hospital, where he was under treatment for some days for chronic rheumatism in the arms and shoulder-joints, contracted during the Crimean campaign. He returned to his barrack-room, in which he remained for the greater part of the day, and was not exposed to the sun. About 3 o'clock he felt heavy in his head, and had pain in the pit of his stomach. This I have noticed to be a premonitory symptom in many cases. He continued in this way, not mentioning it to any one, until about 5, p.m., when he became worse, and then sent for a doolie in order to be carried to hospital. Shortly after his arrival there I saw him; his condition then was:—He was unconscious; skin perfectly dry, and of a burning heat; heart's action and pulse very weak; breathing prolonged and laboured; pupil of the eyes slightly contracted, and

not obedient to the stimulus of light; conjunctivæ much congested. Before I arrived the cold douche was commenced; a purgative enema was given, and a mustard sinapism placed over the heart. The enema acted freely, and I continued the douche, stopping at short intervals, in consequence of the weakness of the heart's action. He continued in this unconscious state until about 9, p.m., when he showed some symptoms of improvement. The breathing became less laboured; he could feel if pinched, or if the soles of his feet were tickled, and could notice an object if drawn across his eyes, but was still unable to speak or answer any question addressed to him. About this time his eyes became more suffused, the scalp was then shaved, and strong vesicating collodion applied to it, and the cold douche continued over the body, as it was still very hot. At 9.30 a violent fit came on, in which he remained for twenty minutes, and during it the pulse was not to be felt, or heart heard. On recovery the heat of the body was much reduced, and the intellect became more clear. By 10.30 he was able to speak, and said he knew not how he came to hospital, and that he now felt no particular pain anywhere, but had general weariness of his limbs and body. He was closely watched during the night. The great heat of body did not return. He got no sleep, but remained in a drowsy state, and had five grains of carbonate of ammonia every second hour. During the state of insensibility this man had twenty mussacks of water poured over him; each of these contained about ten gallons of water.

July 12th, 6, a.m.—Patient complains of weariness; pulse and heart's action stronger than they were; no pain in head or abdomen; the general surface warm and dry, but not very hot. About 10 o'clock some fever came on, his head became hot, and eyes suffused. Leeches were applied to the temples, and a cold douche was administered over the whole body, with benefit. Diaphoretic mixture, with carbonate of ammonia every second hour.

Evening.—Patient better; surface of body cooler, and perspiring; inclined to sleep.

13th.—He slept last night, and feels better this morning; skin is acting. Diaphoretic mixture, with carbonate of ammonia continued.

14th.—Heart's action improved; pulse stronger; skin acting; says he feels quite quiet; slept well through the night. The blister on the scalp is now healing. The mixture before mentioned continued, with five grains of quinine twice a day as a tonic.

16th.—He is going on favourably. No bad symptom that I can

see. Skin is acting, and he rests well at night. The diaphoretic mixture, with carbonate of ammonia and quinine, five grains twice a day as a tonic, continued.

19th.—He is now able to get out of bed, but says his head feels very giddy if he walks, and he is weak on his legs. He says the sensation in his legs is not quite complete, and in walking I notice his gait is peculiar, and he seems partially to draw the left leg after him. Former treatment continued.

24th.—He is gaining strength, and seems to be doing well. Is out of bed, but cannot walk perfectly. There is a peculiar totter in his gait. The lower limbs seem as if suffering from some form of paralysis. Friction to be applied, and other treatment continued.

26th.—He is not so well this morning; he complains of pain and weight in the head, and of an uneasy feeling about the pit of the stomach. The head is not hot, and the skin is acting; bowels are confined. Ordered some opening medicine, and to go on with other treatment as before.

27th.—His bowels were freely acted on, and he says he feels easier, but has yet some pain in his head. His eyes look wild, and his manner is anxious and confused. Scalp to be shaved, and a blister applied to it.

Evening.—Seems easier this evening; complains of no pain.

28th.—He says this morning that since 4.30 (it is now 6.15) he has not felt so well. He is very restless; he breathes with much difficulty; his circulation is very weak, and the heart's sounds can hardly be heard; extremities cooler than natural. A mustard sinapism was immediately placed over his heart, and a glass of wine given him. Carbonate of ammonia, with camphor mixture, to be given every hour. While I stood by his bed, a sharp cramp came on in his right arm and leg, which was relieved by friction with the hand. I heard during the day that he was better.

28th, evening.—He is better this evening; breathes with ease; heart's action improved; has no pain in the head; and intellect is clear. Continue carbonate of ammonia, and keep blister on the scalp open.

30th.—He continues to improve; complains of no pain; and his intellect is quite clear now. He can answer questions quickly when put to him. He is yet, however, very weak on his legs, and drags the left one slightly when walking. The blister to be healed; the quinine, and carbonate of ammonia three times a day to be continued.

August 3rd.—The patient is daily improving in strength, and complains of nothing now but slight giddiness in walking, and weakness of his limbs. His legs are well rubbed daily, and he continues to take quinine.

8th.—He continues to go on well, and complains very little now of giddiness. His limbs are weak, but he is yet able to walk about unsupported, and his skin and internal organs are all acting regularly; quinine continued.

11th.—He is now convalescent. His limbs are getting more firm under him, and he is able to walk about the hospital garden unsupported. He takes at present five grains of quinine twice a day as a tonic, and fifteen drops twice a day of the tincture of the muriate of iron.

The patient has been a very short time in India, and never enjoyed very good health in it. He returns this year to England.

III.—*A Case of Heat Fever, followed by Sun-stroke, with Insensibility; Treatment, and Recovery.*—Private Donald M'K—, age twenty-five, a strong and well-developed young man, of very temperate habits, was admitted into hospital about 11 o'clock, a.m., on the 11th July, 1865. He had returned from guard, was dismissed, and went to his barrack-room, from which he was carried to hospital, suffering from fever. On admission, he stated that for the last five or six hours he had not felt quite well, and that he had some pain in the pit of the stomach, and in his head, but thought it would pass off. It, however, increased, and when he came in, his skin was perfectly dry, and extremely hot. Head burning, heart's action and pulse very strong, and intellect not by any means clear. His clothes were immediately removed; he was carried on a low cot to the verandah, and the cold douche was given to him, which soon got down the temperature of his body, and he declared himself much better. Diaphoretic mixture was ordered for him every second hour, and he remained in a quiet but rather drowsy state for the day. His skin did not become very hot again, at least up to 4 o'clock, and about that time he fell asleep. About 6.30, p.m., he was noticed by the man lying in the next bed to him to make some convulsive movements, and efforts to get out of bed. Very shortly after this I saw him, and found him in the following condition:—Quite insensible; unable to answer any questions, or notice any object drawn across his vision; his skin perfectly dry, and temperature of the surface of body and head extremely high; heart's

action strong, and pulse full, but easily compressed; slight suffusion of the eye, but nothing remarkable about the condition of the pupil—it was immovable, and directed straight forward; he moaned occasionally, and was restless, but had no convulsions. The stream of cold water from a height was commenced the moment his clothes were got off, and that he was placed in the verandah. A strong purgative enema was given, which quickly acted, and carried away a large quantity of very dark and offensive matter; and a sinapism was placed over his stomach.

He continued in this unconscious state until a little after 9, p.m., when he showed some symptoms of recovery. From the time I first saw him, about 7.15, p.m., until this, the stream of cold water over his head and body was almost constant, in consequence of the persistent heat of surface, and as his heart's action and pulse indicated its continuance. He had, during these two hours at least, thirty bheesties' mussacks of water poured over him, each holding about ten gallons. He now noticed objects before him, turned on his side, felt if pinched or if tickled in the soles of the feet, and looked vacantly; when asked questions tried to answer, but could not.

The temperature of his skin went down, and after a little he became slightly cold, so as to require light clothing over him. When put to bed consciousness gradually returned, and by 10.30, p.m., he was able to answer some of the questions put to him, but knew nothing of what had been going on for the last three hours. His head now became a little hot again, and the conjunctivæ became more suffused. I had the scalp shaved, and a blister applied. From the moment he was able to swallow, carbonate of ammonia in five grain doses was given every hour. He remained in a quiet, listless state for the night, but did not sleep much.

July 12th.—Says he feels weary and fatigued this morning, but suffers no pain; has a peculiar uneasy feel about the stomach. General surface warm, not *hot*; skin not acting. A sinapism to be applied to the stomach. Carbonate of ammonia mixture to be continued, and the douche to be repeated should great heat of skin come on during the day. About 11 o'clock feverish symptoms presented themselves, and the cold douche was repeated with relief. I saw him about 5.30, p.m.; he was then very hot, with a dry skin, and intellect very dull, eyes suffused. The douche was again resorted to with benefit, and his skin began to perspire after it. About 8, p.m., his head only increased in heat; three leeches were

applied to each temple, and after them he was much better. Carbonate of ammonia mixture was continued through the night. He rested quietly, but did not sleep.

13th.—He feels weary, but has no pain; skin moist, not very hot; pulse weak. I ordered a glass of wine, and that the ammonia mixture should be continued.

14th.—Says he feels easier, but did not sleep last night. Complains of dizziness and weakness; bowels not moved since the night of the 11th, but he has consumed no solid food. Ten drops of tincture of opium to be added to each dose of ammonia mixture, to give him sleep. Continue wine.

15th.—His bowels were moved. He slept a little last night, and says he feels quite at ease this morning, but there is dulness of intellect, and some hesitation in answering questions. Continue mixture and wine.

Evening.—He had some sleep during the day, but wanders a good deal this evening. He imagines he sees strange objects, and is restless; head a little hot; pulse slightly accelerated, 86. Blister on the scalp to be kept open; another to be put behind the right ear, and tincture of opium to be discontinued in the mixture.

16th.—He was a little restless through the night; he got out of bed twice. He is in a listless, drowsy state this morning, and requires to be roused to answer questions, many of which he answers correctly, and then lapses off into some strange ideas; one is, that he sees himself built up, and he is soon to be complete; another, that he is hunting in Australia. His pulse is quiet, tongue clean, head not hot, skin perspiring, bowels acting. The blister on scalp has healed; another to be put behind the left ear, and the right kept open. Continue the ammonia mixture, and *R. camph. gr. ii.; ext. assafetida gr. ii. Ft. pil. one three times a day.*

17th.—His mind is a little clearer this morning; he answers questions better, but he is not yet well. He suffers no pain, his pulse is quiet, and skin is acting freely. Blisters behind the ears to be kept open, and mixture and pills continued.

20th.—He is much improved, sleeps well, and his intellect is now quite clear. He has no pain anywhere, and all his organs are acting regularly. Omit pills. To have five grains of quinine, twice a day, as a tonic, and to continue the ammonia mixture.

26th.—He is going on very well, and gaining strength gradually. All his internal organs working regularly. Continue quinine.

August 3rd.—Is quite convalescent now; able to walk about, and feeling strong.

7th.—Discharged from the hospital quite well.

IV.—*A Case of Heat Fever, with Partial Insensibility, strongly simulating Sun-stroke, and followed by Head Symptoms; Treatment; Recovery.*—Private James M'B—, aged twenty-eight, a tall man of delicate frame, who has been in India for the last seven years and ten months, was carried to hospital about two o'clock, on the 11th July, 1865, with the following symptoms:—He was in a drowsy, listless, semi-unconscious state, not able to speak or notice anything, until first roused; the head and general surface of the body extremely hot, and perfectly dry to the touch. When asked, he complained of great pain in pit of his stomach, and a giddy, uneasy sensation in the head; his eyes looked drowsy, but there was no injection of the conjunctivæ; the heart's action and pulse were weak.

The cold douche, before described, was immediately resorted to, and, after about ten minutes, restored him to his senses, and reduced the temperature of his skin. He was then put to bed, but still complained of an uneasy sensation about the stomach, for which a mustard sinapism was applied with relief, and carbonate of ammonia, in five grain doses, was commenced as a stimulant. The heat gradually returned to his body, and became very great; at 4 o'clock it was necessary to repeat the douche again; soon after this he complained of pain in the head for the first time, and the conjunctivæ became slightly congested. Six leeches were applied to the temples, which partially relieved the pain; but later in the evening he became very dull and heavy, and raved; his head was then shaved, and blisters were applied behind the ears; the carbonate of ammonia was continued every hour. About 8 p.m. his pulse and heart's action became weaker, and his body generally was then rather cold; he had also a rigor at this time. I then thought it advisable to give him a little warm brandy and water, and had sinapisms placed over his stomach, and to the calves of his legs; his pulse, and the temperature of his body then rose; he rested quietly for the night, and had some sleep.

July 12th.—Says he feels very weary and fatigued this morning, but his intellect is clear, and he is able to answer all questions put to him, but his speech is thick and slow. The history he gives of himself for yesterday is as follows:—About 7 a.m., after morning

parade, he went to the Bazaar, half-a-mile distant, to buy some things; he did not drink anything while there, and was back at 8.15, up to which hour the sun is not very strong; he ate some breakfast at 8.30, and after that was not out of his barrack-room until carried to hospital in a doolie. The first symptoms, he says, he had were pain in the pit of the stomach, and giddiness or lightness of the head. He has a very indistinct recollection this morning of anything that occurred for some hours after his admission into hospital yesterday. He still complains of uneasiness about the stomach, and giddiness of the head. His skin is not acting, and is warm. Pulse and heart's action are still weak. A mustard sinapism to his stomach, the body to be sponged, and diaphoretic mixture, with carbonate of ammonia, to be taken every second hour.

13th.—He is better. The skin is beginning to act, and the uneasiness about the stomach is partially relieved; the head is still very giddy, and speech rather thick. The blisters to be kept open with savine dressing, and the mixture to be continued.

15th.—There is some improvement; his skin is now acting freely; his bowels are quite regular; the uneasiness about the stomach is much better, but his head is yet very giddy, and he is extremely weak. The mixture, beef-tea, and wine to be continued.

20th.—The patient is much improved; he is now able to be out of bed, and can move about without any support; complains only of swimming in the head, and general weakness. Taking quinine twice a day, and the former mixture, with good diet, and a moderate allowance of wine.

He continues to improve in strength and firmness, and was out of hospital, on the convalescent list, by the 25th.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

A Practical Treatise on the Diseases and Infirmities of Advanced Life. By DANIEL MACLACHLAN, M.D. London: John Churchill & Sons. 1863. 8vo, pp. 718.

THE work before us is the result of the experience and observation of one who occupied, during nearly a quarter of a century, an admirable position for studying the diseases and infirmities of the aged; who, in the capacity of physician and senior medical officer of Chelsea Hospital, had upwards of five hundred aged people constantly under his eye, nearly all of them being invalids in the strictest sense, very few below 55 years of age, and most of them varying from 60 to 90 and upwards; and who had from among this number always between 60 and 70, more frequently between 70 and 80 cases actually under medical treatment.

The first or introductory chapter is devoted to the consideration of the anatomy, physiology, and psychology of advanced life; then of the general principles, and the remedial and preventive treatment of the affections incident to age; and finally, of climacteric disease, decay of nature, or breaking up of the constitution.

With regard to the several stages into which the life of man is divided, there is not much room for difference of opinion among physiologists, or pathologists, except as to the exact points through which are to be drawn the lines which separate infancy from youth, youth from maturity, maturity from age. Much ingenuity and refinement have been employed in endeavouring to define the position of these lines of demarcation, but the question for consideration in the chapter before us is limited to one of these stages or periods only, and may be thus defined:—"At what period does advanced life or old age begin, and what are the indications of its commencement?" We quite agree with the author that a candid examination of this question, viewed purely in a medical aspect, is not unattended with many difficulties. Advanced life and old age are relative terms scarcely measureable by length of years. Maturity

slowly and insensibly glides into decline, so that the line of demarcation is imperceptible, and the distinction between these epochs entirely arbitrary, until nutritive and functional inactivity, with structural changes, reveal the degeneration which tyrant time effects in the different organs and tissues. At 45 or 50 many men begin to show indubitable signs of physical decay, though apparently in excellent health, while others, and not a few, more favoured, are vigorous and active at 70. Original conformation, hereditary weakness of the organism and constitution, climate, pernicious habits, anxieties, the privations, disappointments, and heart-burnings incident to particular professions, positions in life and temperaments, tell on the human frame, and occasionally manifest their influence at comparatively early periods. The soldier who has completed one or two-and-twenty years' service in the ranks, and who has not reached the age of 40, deprived of natural rest, leading a monotonous life, and breathing the tainted air of a crowded barrack room, already shows in his wan and withered countenance, and by his failing strength and activity, the tear and wear of his constitution. Artizans, similarly circumstanced, evince the same indications of physical decay, while as yet young in years; and hardly a day passes without the observer meeting old men at 50, and decrepitude at 60. This is not, however, the ordinary course of nature, but a premature old age and decay, hastened and flowing from one or more of the causes now hinted at. The discharged soldier regains the freshness and bloom of manhood, and the lawyer, the official, or merchant, enjoying an easy and dignified retirement, loses, with his anxieties, cares and responsibilities, the mien so well known, that bespeaks the commencing return of the body to the earth which gave it.

Having treated very fully, in the next part of this chapter, of the somatic modifications and changes, physiological and physical, which characterise the advance of life, the author proceeds to consider the state of the moral and intellectual faculties. If the aged lose their corporeal vigour and activity, and are exposed to many infirmities which afflict the body and rob it of some of the pleasures of other years, the understanding and the higher powers of the mind, up to a certain epoch at least, and often to the very latest period allotted to the life of man, preserve, if they do not actually acquire, increased energy and precision. More slow in reaching maturity, reason and judgment have scarcely attained their highest development before the signs of bodily decay appear. Advanced life, and

consequent decay of the organism, are not necessarily productive of, or accompanied by, inactivity of the mind—dotage or decline of the intellectual faculties. These are the last to yield under the pressure of years, and, as relates to the body, the inexorable law of nature.

In speaking of the duration of human life, the author takes occasion to direct attention to a remarkable fact which has not been sufficiently recognised, namely, the extreme rarity of death from sheer old age. The mortality tables, published under the direction of the Registrar-General, would appear to tell a very different story, and the explanation of this is furnished by the fact that in very numerous instances of deaths occurring *in* old age, the morbid processes which have brought about the fatal result were so obscure in their nature, and attended by symptoms so unlike to those intense and strongly-marked manifestations which are wont to characterise the presence of disease in the earlier periods of adult life, that their operations were unperceived in some cases, while in others they were interpreted rather as the exponents of a natural process of decay; and in this way such terms as atrophy, senile decay, asthenia, and old age, have come to be very often erroneously assigned as the “cause” of death in medical returns. In the second chapter the author gives us an abstract of the results of his investigations into the causes of death in all the cases which occurred in Chelsea Hospital during the thirteen years ending with 1859. Out of a total number of 845 deaths, exclusive of sudden deaths investigated by a coroner’s inquest, occurring among the in-pensioners whose ages varied from 50 to 90, the immense majority exceeding 60; and in all of which the greatest pains were taken to return the true cause of death, the diagnosis having been verified in every doubtful case by *post mortem* examination, the several diseases are reported to have been present in the following proportions:—Bronchitis was the cause of death in 166 cases; phthisis pulmonalis in 145; apoplexy, 58; pneumonia, 51; cancer, including scirrhus, of different internal organs, 47; diarrhea, 36; empyema, 27; hemiplegia, 25; pericarditis, 24; cholera, 23; softening of the brain, 21; diseases of the heart, hypertrophy, dilatation and valvular lesions, 21; albuminuria, 17; diseases of the bladder, cystorrhœa, &c., 17; general paralysis, 11; dry gangrene, 10; paraplegia, 7; erysipelas, 7; sloughing of the nates, 7; purpura and scurvy, 5; “old age, 3; all 82 years old. Including two cases of death by angina pectoris, and one by gout, the remainder are scattered; but these constitute the

chief numbers. The reader will not fail to remark the large number of phthisical cases."

The total or partial exemption of the aged from many diseases which prevail in earlier life forms the subject of some interesting remarks, in the course of which it is stated that "strumous and tubercular affections, with the exception of consumption itself, which, as we have just seen, is far from uncommon, are comparatively rare." We cannot say that we coincide with the author in this opinion, our experience being rather in favour of the belief that strumous disease affecting the joints and the glands, as well as that condition which may be denominated strumous cachexia, is by no means rarely met with after the age of fifty-five years; in fact, we can call to mind very numerous instances in which we have met with the most decided manifestations of strumous disease among the aged and infirm, exclusive of cases of pulmonary tubercle.

That the author has studied well and considered carefully the principles upon which the medical treatment of the aged should be conducted, is evident from the judicious tone of his remarks on the principal remedies. The young and inexperienced practitioner is reminded of the importance of paying due attention to other circumstances than the mere number of years his patient may have attained, inasmuch as this in itself does not constitute old age; on the contrary, he must weigh a number of circumstances, which no arrangement of figures can represent—circumstances relating to constitution and habits, amount of vital power, persistence of depressing or debilitating agencies, hereditary or family peculiarities, and so on. The decline of the vital powers, whether it take place at an early or a late period, and the degeneration of the tissues which accompanies and attests this condition of decadence, materially influence the indications of treatment and modify the effect of medicines, which, as a general rule, become less certain in their immediate operation, and less efficacious in their specific qualities or ultimate effects, as old age advances. The following remedies, which are among the more important which are employed in the treatment of disease, are reviewed by the author with reference to their adaptation to the aged. We append a brief abstract of his observations.

With regard to bloodletting, Dr. Maclachlan is rather disposed to consider that old age, *per se*, should not deter us from employing this valuable remedy. He thinks the principle of trusting to the expectant method of treatment is often carried to a dangerous extent,

and that those persons are in error who allege that the constitution of old people, and their intolerance of bleeding, assimilate with the condition of early infancy; for while, in the first stages of life venesection cannot be resorted to with safety, and must give place to local bloodletting; in the other extreme, if not carried to excess, it is well borne when the great functions of life are still performed with a certain degree of vigour, and the heart retains its normal condition. The authorities who are quoted as favourable to the views above stated are numerous and valuable; while amongst those who have maintained an opposite opinion, not a few names of eminence are enrolled. The author concludes with the following judicious remarks:—

“Amid so much rashness on the one hand, and extreme caution or timidity on the other, the question naturally occurs, what should be regarded as the safest indication for bleeding in old age? and, as a corollary to this question, what are the circumstances that ought to forbid it? Bleeding may be generally employed when the danger or urgency of the symptoms call for it—when the individual, however far advanced in years, still possesses some of the attributes of maturity, the pulse at the wrist and in the other larger arteries being firm and equable, the action of the heart regular, and the constitution, though perchance debilitated by natural decay, unbroken by pre-existing chronic disease. General bleeding is contra-indicated when the pulse is irregular in strength and frequency, but more especially in the former state; when the impulse of the heart is feeble and intermittent, and its sounds obscure and inaudible; and in all cases where the general health has suffered from privations, insufficient nourishment, or long-continued exhausting maladies. In practice, the cases in reality are comparatively few in which the abstraction of blood from the arm more than once or twice is necessary. . . . In considering the expediency of a first or second bleeding, the practitioner should not be deceived by the hardness of the pulse at the wrist in the old, which, as Bizot has pointed out, is often stronger in them than in the young, owing to the radial artery preserving a sound condition, and thus receiving the entire force of the heart’s action, which is partly spent in dilating the whole system of arterial tubes, when all possess their normal elasticity before the changes effected by age occur in them. The action and impulse of the heart itself are safer guides than the pulse at the wrist; and we hardly ever bleed old people by the lancet without first laying our hand on the cardiac region, and, if need be, applying the ear to the chest.”

The author has found purgatives in many cases to be a most

valuable substitute for bleeding; they are, however, less immediate in their results; but if too active, or too frequently repeated, they are apt to depress the vital powers even more than a moderate depletion by the lancet. With a view of relieving the bowels speedily without exhausting, a combination of a few grains of calomel with the compound extract of colocynth is recommended. In cases of bronchitis attended with sudden clogging of the lungs, congestion, œdema and suppressed or inefficient expectoration, "we know of no purgative, indeed of no remedy, so useful as the pulvis jalapæ compositus. These most distressing cases, with universal moist râles and great accumulation in the bronchi, intense dyspnea and lividity of the face—symptoms betokening suffocation—are often singularly relieved by means of this remedy. It not only procures copious serous motions, but also acts on the kidneys, and promotes absorption." To say the truth, we prefer to treat those cases of suffocative catarrh by a less depressing method, namely, by exhibiting, in the first instance, a *full* emetic of mustard, with carbonate of ammonia, which speedily unloads the respiratory tubes; afterwards by diffusible stimulants, and in the meantime to keep up a moderate counter-irritation by means of turpentine applied not only to the thoracic surface, but also to the legs and feet. We quite agree as to the great value of cathartics in the apoplectic and comatose affections of advanced life, as well as in temporary attacks of senile insanity. Mercury is considered "as uncertain in its operation and effects, and not without more or less inconvenience, or even danger, when given for some time in repeated doses to the aged. As a purgative, calomel is a safe and most valuable remedy at this period of life; but as an alterative, mercury, in any form, is hazardous. When exhibited in this manner its effects should be carefully watched, for it is frequently difficult or impossible to foresee them."

The tartrate of antimony, when employed as a counter-stimulant, so as to maintain nausea and diminish the strength and frequency of the pulse, is looked upon by the author as a dangerous remedy in the old; but he does not apprehend that injuriously depressing effects will attend the administration of pulvis Jacobi verus, from which, in combination with small and regulated doses of calomel, much benefit has been found to arise in many inflammatory affections. In cases where we are desirous of reducing the pulse without more direct means, ipecacuanha is recommended as being preferable to the tartrate of antimony.

Emetics are admitted to be occasionally advantageous in some

bronchial affections in which the respiratory tubes are loaded with viscid secretion; also to be appropriate remedies in cases of gastric derangement from the presence of excess of undigested food. Their employment, however, in either of these classes of affections is not spoken of in very strong or glowing terms of recommendation.

Opium requires to be prescribed with caution in aged and debilitated subjects. Although, generally speaking, the diseases of advanced life are not accompanied with acute suffering of a nature to be relieved by opiates in unusual quantity, there are, nevertheless, many cases in which pain is a prominent symptom, and in which the continuance of excruciating torture becomes attended with a gradually-increasing tolerance of narcotic drugs. Such cases, however, must be looked upon as exceptional; and it were well to bear in mind, as a general rule, that "nothing but the most urgent necessity ought to induce the practitioner to exceed ordinary doses of half a grain or a single grain, wherever, through natural causes, the powers of life are perceptibly declining, and a state of exhaustion has been brought about by persisting chronic disease."

Remedies of the diuretic class are of necessity often resorted to in the diseases of advanced life, owing to the frequency of dropsy in one form or other, either as an attendant or a sequela of the numerous maladies that assail the aged. When the skin is altered in its structure with advancing years, and unperspirable, they take the place of diaphoretics, which often fail in their object. Free action of the kidneys has also a powerful effect in relieving the lungs and brain; and in heart disease, with its many complications, diuretics are sometimes of great benefit. Nitrate of potash is highly spoken of, but not in large doses; also the acetate of the same base. The author combines one or other of these salts, or both, with solution of acetate of ammonia and sweet spirit of nitre, allowing at the same time a moderate quantity of gin and water where a stimulant is necessary. Digitalis has been found to be "a truly valuable medicine. Where there is no sign of fatty degeneration of the heart, or where the heart's action is not feeble and fluttering, it may usually be given with perfect safety, even when the pulse is intermittent—a symptom which sometimes disappears under its influence."

Considerable diversity of opinion exists as to the safety and utility of administering colchicum to persons of advanced age, whether the effect intended to be produced be diuretic, antiphlogistic, or antilithic. "Like other powerful remedies, its operation

requires watching. A careful and judicious administration of it, in moderate or medium doses, is occasionally attended by the best effects in the very decline of life. It may in this manner be given to nonagenarians without any apprehension; but as it is one of those medicines that accumulate in the system it should occasionally be intermitted when it is considered expedient to discontinue its administration for a length of time; and in all cases, immediately it is observed to affect the pulse or attack the bowels, it should at once be omitted. The best guide to its safety is the precise condition of the heart."

Squill, and its preparations, long used as diuretics and expectorants, are highly esteemed by the author in senile affections. He does not omit to notice an inconvenience which attends the prolonged administration of this valuable drug in some cases, and which is a serious impediment to its use—namely, an impairment of the tone of the stomach, evinced by a failure of appetite and a tendency to nausea. A similar drawback to the use of digitalis has also been noticed.

Amongst the tonics the practitioner has generally a pretty wide range of choice, where medicines of this class are indicated; and truly they are found of service in some stage of almost every disease at every period of life. The combination of aromatics and carminatives with vegetable bitters is a matter of importance to be attended to in many of the affections of old age. Quina is of less value here than in earlier life. Minerals, such as the acids, and some of the preparations of zinc and iron, have been frequently found to be of service. Arsenic is not so strongly recommended.

"Extreme caution" should attend the administration of hydrocyanic acid to the aged. "It has often been a matter of surprise how old people ever get over the large doses of this most potent remedy which certain practitioners appear to be in the habit of prescribing with a recklessness that betokens profound ignorance of its depressing influence on the nervous system, and its extreme danger."

Strychnia and veratria also, and other alkaloids of this kind are spoken of with similar reserve as to the safety and propriety of their administration.

Among the safe and more useful remedies in constant requisition are enumerated those of the revulsive class, such as sinapisms and turpentine epithems. Lastly, balsamic substances are most favourably spoken of as extremely beneficial, especially in catarrhal

affections, not only of the genito-urinary but of the pulmonary mucous surface.

The concluding section of the second chapter is devoted to a consideration of the hygiène of the aged, and presents a clear and concise abstract of the principles on which our sanitary directions should be based.

The next chapter treats of climacteric disease, decay of nature, or the breaking up of the constitution—a disease whose existence has been denied by some. while its frequency and importance are said to have been exaggerated by others—a disease which was regarded by Sir Henry Halford as solely one of advanced life, occurring with the changes in the system in age so very irregularly that it might occasionally be remarked at any time between fifty and seventy-five years of age—an affection which, according to the views of Dr. H. Kennedy, of this city, “is by no means unfrequently met with in individuals between twenty and thirty years of age.” In general terms, the state of things to which Sir H. Halford (in his valuable essay in the fourth volume of the *Trans. Coll. Phys., London*, for the year 1813) first applied the term “climacteric disease,” may be considered as a “rapid decay of the system more closely resembling morbid action than the almost imperceptible and silent changes of a physiological nature effected in the economy by the progress of years.” Our author’s own opinion is that the disease in question is rarely met with as an idiopathic or distinct affection. “Many years ago,” he writes, “imbued by the description given of it by Sir Henry Halford, I imagined that I had met with it in numerous instances, but I soon found that in those cases terminating fatally, and which I had set down in my own mind as climacteric decay or disease, death was the result of some latent affection of long standing, hidden from observation by the absence of its ordinary symptoms, the most frequent being tubercular disease of the lungs, organic disease of the liver, or cancer of one or more of the internal organs. And in cases terminating favourably, recovery took place after the subsidence of a coexisting malady, such as a dyspeptic, bronchitic, or gouty attack. It is the engrafting of some malady on the senile constitution, premature or delayed, that forms a part, and no unimportant part, of the disease. I am satisfied that, until of late years, granular degeneration of the kidney, occurring in elderly people, without anasarca, must have frequently been put down as climacteric decay.”

Reference is frequently made throughout this chapter to the paper of Sir Henry Halford, from which the author extracts freely; numerous passages also are quoted from the valuable monograph of Dr. Henry Kennedy on the same subject, which communication was printed in the twenty-fifth volume of the *Dublin Journal of Medical Science*, former series (1844). This writer has entered more minutely into the history of the disease, but in the main his views coincide with those of the former as respects its usual gradual mode of invasion, the character of the symptoms, diagnosis, and treatment. We conclude our notice of this part of the work by inserting the following passage, which occurs in a letter to Sir John Sinclair, by Professor Waterhouse, of the University of Cambridge, U.S. It is inserted by Dr. MacLachlan in order to record the fact that about six years before the appearance of Sir H. Halford's essay already referred to—namely, in the year 1807, the period and symptoms of “climacteric disease or decay” were clearly pointed out by the writer above named:—

“There are certain periods of life, if I mistake not, which are scarcely noticed by medical writers—viz., about the age of thirty-six, when the lean man becomes fatter, and the fat man leaner. Another, between the years of 43, 44, and 50, when his appetite fails, his complexion fades, and when his tongue is apt to be furred on the least exertion of body or mind. At this period his muscles become flabby, his joints weak, his spirits droop, and his sleep is imperfect and unrefreshing. After suffering under these complaints a year, or perhaps two, he starts afresh with renewed vigour, and goes on to 61 or 62, when a similar change takes place, but with aggravated symptoms. During the natural change that takes place between 43 and 50, no particular organ suffers, but a gradual and uniform *deterioration* supervenes. At this time he first experiences a reluctance to stoop, he prefers a carriage to riding on horseback, and he finds himself more affected by changes of the weather. He nevertheless commonly passes through this kind of ‘*moulting*,’ and regains his health, with a little diminution of muscular strength, until he turns 60; then the gravity of age is more strongly marked, and he begins to boast of his age and its prerogatives. This is the result of my observations on others, compared with my own personal experience.”^a

The next portion of the work treats of diseases of the nervous system, according to the following arrangement: commencing with some introductory remarks on the anatomical characters of the

^a Code of Health and Longevity, Vol. i., p. 33.

nervous centres and their investments, and general observations on diseases of this part of the organism in advanced life; the author then proceeds to take up the following affections in succession, viz., meningitis; hydrocephalus, and cerebral œdema; vertigo; epilepsy; apoplexy; meningeal apoplexy; softening of the brain; hemiplegia and paraplegia; paralysis agitans.

In the chapter on meningitis, we find some useful and practical remarks on the acute form of this disease—an affection which is too often looked on as belonging exclusively, or at least principally, to early life, but one which is really of very frequent occurrence in the aged. The important error of allowing it to be confounded with, or mistaken for, some of the forms of low fever is clearly and forcibly shown—an error which is apt to be committed by a young practitioner, who might easily be led astray in his diagnosis by the absence of intensity, which characterises the early symptoms, as well as the insidious mode in which the disease commences. The whole description of the symptoms of the affection in question, and its diagnosis from typhus, evince the most careful clinical observation on the part of the author. With respect to the cause of acute meningitis, setting aside those of a mechanical nature, such as injuries of the head, diseases of the bones, and morbid growths, Dr. MacLachlan is disposed to attribute the origin of the affection, in the great majority of cases occurring amongst old persons, to one or other of these three sources—rheumatism, gout, or albuminuria.

The reader will find an account of the treatment in the following extract:—

“Treatment.”—It very rarely happens that the symptoms of the disease are of so intense a character as to demand general bleeding, but in vigorous constitutions this measure is sometimes necessary, and venesection may then be employed with benefit. In illustration of this, I may mention the case of an in-pensioner of Chelsea Hospital, sixty-eight years of age, muscular, thick-set, and healthy, who was admitted into the infirmary thereof in the summer of 1855 with all the symptoms of meningitis, accompanied with high mental excitement, flushing of the face, brightness and restlessness of the eyes, heat of the scalp, perpetual loud talking with laughter; incessant efforts to get out of bed; occasional rigid extension of the index finger and contraction of the forearm, particularly strong on attempting to bleed or control him. These symptoms had set in the preceding evening. His pulse was quick and full, the tongue clean. Bleeding was performed to the extent of sixteen ounces, the blood flowing in a large, remarkably florid stream, with but slight

impression on the action of the heart. It had, however, the effect of subduing the mental excitement and reducing the redness of the face. A few hours afterwards he was much more rational, less talkative, and the pulse had fallen from 120 to 100. Now, as yet, his bowels had not been opened, and the only medicine given him was the sulphate of magnesia in the infusion of senna, so that the improvement was justly due to bleeding. On the following day he was still better, though suffering from frontal headache, with suffusion of the eyes, &c. The disease pursued a mild course, and the man perfectly recovered. It is more than likely that in this case the inflammation of the membranes was associated with inflammation of the periphery of the brain from the commencement. Of the existence of phrenitis, in the common acceptation of the term, none of the medical officers had ever any doubt.

“The examples, however, in which bleeding from the arm is obviously necessary and expedient, are nevertheless few in comparison to those in which such a proceeding would be improper; for though at the onset the prostration accompanying the disease is often more apparent than real, but a short time elapses before the symptoms assume a character to forbid active treatment. In by far the greater majority of cases, local must be substituted for general blood-letting, even at an early period of the disease; and in its advanced stages, opening a vein would hasten a fatal termination. Except in such an instance as I have alluded to, attended by high cerebral excitement and vascular action, local blood-letting is not only infinitely more safe, but more beneficial, and it can be repeated from time to time, as the nature of the symptoms may indicate, while general blood-letting can very rarely be resorted to more than once, and that only at the commencement of the attack. The beneficial effects of local blood-letting may be greatly aided by position, and the constant application of cold to the head.

“No time should be lost in opening the bowels. Four or five grains of the compound extract of colocynth, with two or three grains of calomel, should be given, and followed up in a few hours, in uncomplicated cases, with salts and senna. In feeble, emaciated, or exhausted habits, care should be taken not to act too sharply; but, where purging can be borne, the saline aperients are of great service. Mercury appears to have a beneficial influence in arresting the progress of the disease, and averting its consequences in uncomplicated attacks, not dependent on gout or granular degeneration of the kidneys. A grain or two of calomel should be administered every two or four hours, and the mouth, if possible, very gently touched. Where local bleeding is not repeated, the calomel may be conjoined with James’s Powder, or fractional doses of tartar emetic. Sufficient care must be taken not to depress the vital powers below a just relation with the activity and nature of the existing symptoms. Having this in view, it will also in general be advisable to allow a fair proportion

of nutriment, milk, strong beef-tea, sago, &c., from the beginning. Although light and noise may not disturb the patient, the room should be darkened and quiet maintained.

“When the more active symptoms are subdued, and stupor has succeeded excitement, a blister should be placed on the nape of the neck, and the raw surface afterwards dressed with blue ointment. Blistering ought invariably to be delayed till the period of active excitement has ceased or abated. Cold should still be continued to the head, alterative doses of calomel or blue pill administered, the bowels regulated, and the strength supported by mild unstimulating nutriment. During convalescence, and in certain cases characterised by nervous irritability, sedatives, especially henbane, or even the muriate of morphia, may be prescribed with great benefit.

“In the secondary forms of the disease succeeding albuminuria, our chief resources are, leeches to the mastoid processes, cold to the scalp, and blisters to the nape of the neck. Mercury is then of little or no avail; indeed it is prejudicial. When the disease accompanies or follows gout or rheumatism, very similar measures are required; in addition to which we should endeavour to maintain or elicit the original affection by mustard cataplasms and tepid pediluvia, while at the same time the poison itself should be destroyed or its formation prevented by the internal exhibition of colchicum with alkalies.”

We shall not dwell on the remaining chapters of this part of the work, but pass on to the next, having for its subject matter the consideration of the principal diseases which affect the respiratory organs.

The author commences by giving a brief account of the important changes of structure and modifications of form, which in the advanced stages of life are found to occur in these organs, and in the walls which enclose them. Commencing with the larynx, whose greatly enlarged cavity, increasing rigidity of cartilages, and pale and flabby condition of vocal muscles, are alluded to; the state of the trachea is next noticed as one of dilatation and ossification, although the former condition is not so common here as in the larynx; Rokitansky, however, says that it always occurs with senile marasmus of the lungs, and is more or less proportional to it. The great prevalence of an abundant secretion from the pulmonary mucous membrane, which has been specially noticed by MM. Hourmann and Dechambre, as the habitual physiological condition in the aged, Dr. Maclachlan has found to be frequently absent, even in octogenarians. The most important changes which take place in the lungs themselves have been long since described with the most

careful minuteness by Lænnec, the accuracy of whose account has been confirmed by the researches of MM. Hourmann and Dechambre, published in the *Archives Gen. de Med.*, for August, 1835; while on those researches is based Rokitansky's account of the anatomical characters of the thorax and lungs of the aged. Frequent reference is made in the present work to the writings of the several authors just named.

In connexion with the physical signs, whose consideration immediately precedes that of the special diseases of the respiratory system, the author very judiciously reminds us that—

“While in young children the respiratory movements are chiefly executed by the diaphragm, and in adults equally by the intercostal muscles and diaphragm, in old age they are mainly effected by that muscle. And while the enlargement of the chest during inspiration takes place in every direction in the adult, this enlargement is chiefly vertical in the aged, by reason of the anatomical changes in the bony walls of the cavity, which almost entirely prevent expansion in the lateral and antero-posterior diameter. Excluding cases in which the respirations are either remarkably slow or remarkably frequent, the average number of respirations may be stated to be twenty-one or twenty-two in a minute in healthy subjects above sixty years of age. In adults they are rather under that number. Their rhythm, according to Dr. Sibson,^a in the perfectly tranquil breathing of adults is equal, or as 6 to 6; but, in old age, he states, expiration is prolonged, and inspiration is to expiration as 6 to 8 or 9. The inspiratory movements in the aged are frequently interrupted, or jerking, resembling successive sighs, as if the air, in expanding the lungs or enlarging the chest, met with so many obstacles in its course. This variety of breathing is often seen in perfectly healthy elderly people, though more frequently in diseased subjects, and is a not uncommon attendant in senile pneumonia. So marked is it at times, even in health, that a casual observer can hardly escape noticing it.”

The special affections treated of in the succeeding chapters of this part of the work are:—Acute bronchitis; chronic bronchitis, catarrhus senilis, chronic catarrh, or Winter cough; bronchorrhea, pituitous catarrh, pituitous phthisis, catarrhal phthisis, bronchial flux; pneumonia; chronic pneumonia; pleuritis; phthisis pulmonalis, including the acute, the latent, and the chronic forms; and lastly, asthma.

^a M. C. Trans., Vol. xxxi., p. 378.

The chapter on pneumonia is remarkably full. Speaking of the frequency of its occurrence, Dr. Maclachlan observes that—

“Of the inflammatory diseases affecting the respiratory organs, the frequency of pneumonia at advanced epochs of life is only exceeded by bronchitis, of which it is very often a consequence; and of all acute diseases, with this exception, and excluding also, as sometimes rapidly fatal, apoplexy and palsy, it is that which destroys the greatest number of old people. Often insidious in its origin and progress, it is occasionally altogether latent, and death is not unfrequently attributed to other causes in old age when pneumonia has chiefly or entirely occasioned it. Pneumonia carried off in England and Wales in the seven years 1848–54 no fewer than 24,572 persons, male and female, of forty-five years of age and upwards, the mortality increasing with age. In a *population* of 539 old men in Chelsea Hospital, whose age varied from fifty-five to ninety-five and upwards, it was the undoubted cause of death of 48 of that number, during the fourteen years ending 1859. ‘At the Bicêtre, from 1832 till 1835, pneumonia formed very nearly a ninth part of the cases treated in the wards of that hospital, and about a sixth of the deaths observed in the same period.’^a ‘I believe I speak the truth,’ says Cruveilhier, ‘in affirming that five-sixths of our old women at the Salpêtrière die from pneumonia.’^b Every circumstance connected with a disease of such frequency and fatality is therefore deserving consideration; and the immense importance of the subject will, in the estimation of most readers, be held as a sufficient reason for treating it here methodically, and as the only apology necessary for indulging in details some of which possess little value in the eye of the practical physician.”

Adapting, to a certain extent, the excellent description of the anatomical characters of pneumonia which Lænnec has drawn, and the divisions into stages or degrees pointed out by this very accurate observer, our author directs attention to the modifications in the character of these different stages by reason of the rarefied condition of the lung in the aged, also its increased lightness, the dilatation and rupture of the air cells, and the presence and diffusion of carbonaceous matter through the pulmonary substance. These conditions, he observes, “modify the character of the different stages, sometimes in a remarkable degree, and specially affect the second and third stages of the disease, in a manner peculiarly distinguishing the red and grey hepatization of old age from the same stages in the adult, so that it is often as easy to point out the lung

^a Prus. Mém. de l'Acad. de Med., Tome viii, p. 1.

^b Anat. Path., Tome ii.

of an old person attacked with inflammation as it is to discriminate between the healthy lung of the different epochs of life." There are also some excellent practical remarks on the obscure and latent character which attacks of pneumonia very frequently possess in the aged, especially in cases where chronic disease of the brain, heart, or some other internal organ, has already existed.

With respect to the treatment of the primary sthenic, or most simple form of the disease, Dr. Maclachlan attaches much value to the abstraction of blood, under certain restrictions. He enters fully into this very important question, and sums up in the following words:—

"Although I have recommended bleeding in sthenic pneumonia occurring in vigorous constitutions, irrespective of age, still I feel bound to observe that the cases requiring and admitting it are in reality few. Occasionally a succession of cases occur in which bleeding appears to be imperatively demanded; but, on the other hand, still more frequently, we dare not bleed. Of late years these cases have been increasing. A low form of pneumonia, not exactly of an adynamic type, is more common than it used to be, and for one case now suited to bleeding, we meet with at least a dozen that would be injured by it. In the autumn of 1860, and winter of 1860–61, out of at least twenty cases, I have not once bled, or felt called upon to bleed. My views of the proximate nature of the disease are not changed. It is not here as in apoplexy, in the pathology of which our knowledge has greatly extended. I believe that if there is any disease more than another likely to be benefited by blood-letting, or which pathologically and physiologically suggests and warrants this measure more than another, it is acute sthenic pneumonia occurring in sound constitutions. Inflammatory diseases have changed their type, or the powers of the system are now-a-days less vigorous than formerly, and succumb more speedily, decidedly, and unresistingly, to loss of blood and active treatment. Thirty years ago and upwards, bleeding was the sheet-anchor in most inflammatory diseases, and in none more extensively and more successfully employed than in pneumonia; but disease has assumed a different character, and the attentive observer knows full well that, as Sydenham long ago pointed out, there are certain years and certain seasons in which active measures, and blood-letting in particular, can neither be had recourse to with benefit nor safety. We are now in this cycle or 'constitution.' It is protracted, and has long been oscillating, though seemingly ever advancing; and if it go on advancing, the present generation will live to see bleeding expunged from the list of remedies in this as in most other diseases. The next may see fit to revive it."

Emetics also are favourably spoken of; but the tartrate of

antimony, in large and repeated doses, is not recommended. The author, however, considers this preparation in doses of one-sixth or quarter grain to be of value as obviating the necessity of repeated venesection in sthenic attacks, and superseding this measure in cases of doubtful or less acute character. The addition of nitrate of potash to minute doses of the antimonial salt is recommended as a "safe, salutary antiphlogistic," owing to its calmative influence on the excited circulation, and its tendency to determine to the kidneys. Much value is attributed to mercurials in combination with opium, but "no more opium should be admitted into the combination than is sufficient to prevent the calomel from running off by the bowels." As a general rule, vesication over the site of the inflammation may be employed at an earlier period than in the adult; but in cases of associated bronchitis, occupying a great extent, turpentine epithems are recommended as more convenient and useful than blisters. The propriety of supporting the strength by appropriate nourishment throughout the progress of the case is strongly insisted on, as well as the necessity of allowing such an amount of vinous and other stimulants as signs of debility may indicate, the quantity being regulated by the previous habits of the patient.

The chapters on the remaining affections of the respiratory organs contain some valuable observations and suggestions, on which, however, we shall not now dwell, but pass on to examine the portion of the work which treats of diseases of the circulatory organs.

In the introductory chapter, in the course of some general anatomical considerations, the author remarks:—

"In numerous cases in old subjects I have found the base of the aorta and pulmonary artery externally just as they leave the heart, and sometimes for some little way upwards, minutely injected. The reflected portion of the pericardium has then appeared looser, the sub-cellular tissue relaxed, and the arteries and veins ramifying in it larger and more tortuous than natural. Hitherto I have been unable to connect this appearance with any particular mode of dying, though it has generally but not invariably presented itself in chronic pulmonary affections. It has never been accompanied with any local signs of inflammation, and it has always appeared to consist in a varicose enlargement of the vessels, ramifying in the cellular coat of the arteries in the situation stated. Occasionally, it is limited to either artery. More frequently, it shows itself around the commencement of the aorta alone. It is just possible

that it might be mistaken for inflammation of the cellular tunic of the vessel."

Ever since the time of Hunter, who first directed attention to the peculiarity of the valves at the right side of the heart, the competency of the tricuspid to discharge its functions has been recognized to depend on the incompleteness of its closure. For an ample confirmation of this anatomical fact, as announced by Hunter, we are indebted to the observations and experiments of Mr. Adams and Mr. T. W. King. The insufficiency of the valves of the right side is likewise regarded by Dr. Latham as an admirable provision, coming into frequent exercise in health, and absolutely required to prevent the gravest injuries, and even to guard the continuance of life. This peculiarity, although not confined to the old, is more remarkable in them. In speaking of the venous pulsations so often seen in aged patients, Dr. Maclachlan says:—

"Consequent to this imperfection, so to speak, of the tricuspid valves and the greater dilatation of this orifice in advanced life, pulsation of the jugular veins is more frequently seen than; and in many old emaciated subjects any undue excitement of the heart is productive of undulation of the veins above the clavicle, sometimes extending beyond that. Occasional distention and tremor of the jugular veins may thus be regarded in general as of less serious import in the aged. When the vibratory motion extends beyond the neck, when it reaches to the veins of the extremities, and is permanent, the dilatation of the tricuspid orifice is abnormal, and the evidence of disease of the right side of the heart conclusive. Venous congestion, if not already urgent, is at hand, and hemorrhage or dropsy not far distant. Tricuspid regurgitation is seldom accompanied with any murmur. Although mere hypertrophy of the heart may occasion undulation of the jugular veins, this is, nevertheless, the most direct sign of insufficiency of this valve, and, as might *à priori* be expected, the pulsation is more obvious on the right than on the left side. The veins here are often prominent and undulating, while those on the opposite side are not more than visible."

In the course of this same chapter we are furnished with a careful *résumé* of the observations of MM. Leuret and Mitivié, of MM. Hourmann and Dechambre, and of Dr. Pinnock of Philadelphia, on the subject of the frequency of the heart's action and pulsations in advanced life. The labours of these several observers, with whose general results those obtained by our author coincide, tend to establish the fact that, contrary to pretty general belief, the average

number of the heart's pulsations in the minute is, if anything, greater than in the young adult, or at the middle period of life. "It is in old age," say MM. Hourmann and Dechambre, "that the pulse presents extremes of slowness or of frequency; but the first case is the exception, the second is the rule; the error of past time has been to take the one for the other." Dr. Maclachlan concludes, from his own observations on aged men, coupled with those of the writers above named, that, "as a general rule, it may be stated, in healthy subjects, the number of the pulsations of the heart in the minute increases with the advance of life after sixty years of age, though slowly; in the aged female, as in the adult, it is quicker than in the male of the same age, and in both it gains four or five beats or more when the person is standing. In this respect change of posture seems to have as much influence, if not more, on the heart's action in the old."

So much has been written during late years on the subject of cardiac affections that in a work like this before us we can hardly expect to meet with much that is new. We have, however, carefully examined the several chapters on the different organic and functional derangements of the viscus in question, and find that, as in the former parts of this work, the author has brought together in a most satisfactory way the facts which former labourers had collected, with a view of establishing the general principles of treatment referable to the heart itself, and to the effects of its lesions on the system; and we would observe that the chapter on angina pectoris is specially worthy of perusal.

We must pass over the next portion of the work—that which treats of diseases of the digestive organs—notwithstanding that it presents much that is interesting, especially as respects the chronic and symptomatic forms of dyspepsia.

Diseases of the urinary organs form the subject of the next part. Want of space forbids us to enter as fully into the consideration of the chapter on those affections as we would wish. We extract, however, the following important observations on chronic enlargement of the prostate gland, as we believe that many have fallen into error in estimating the frequency of this affection among the aged:—

"Forcing itself upon our attention by the sad train of symptoms accompanying it, chronic enlargement of this gland has long been recognized as a malady of great frequency and extreme importance. It is a rare occurrence, says Home, for a man to arrive at eighty years of

age without suffering more or less under it; and so common has it been supposed, that one of the greatest of British surgeons, lately passed away, seemed to regard it as an invariable accompaniment of old age. It may safely be asserted, however, that nine-tenths of those who reach the age of seventy and upwards die without ever having exhibited any symptom of prostatic disease. The accurate observations of Mr. Henry Thomson, on the 'Anatomy and Pathology of the Adult Prostate,' in which he shows by statistical research the fallacy of the usually-received opinion that enlargement of the prostate is one of the changes natural to old age, have been corroborated by the still more extended inquiries of Mr. Cockburn Messer, in his 'Report on the Condition of the Prostate in Old Age,' founded on the dissection of 100 specimens in individuals over sixty years of age, by which 'it appears that 35 per cent. of all prostates after the age of sixty are abnormally large, 20 per cent. are abnormally small, and 45 per cent. are within the limits of the normal weight.' Mr. Thomson found an appreciable enlargement at the rate of 32 per cent. in persons above fifty years old, and notable enlargement, causing symptoms during life, at that of 12 per cent. Long before these results were obtained, I had satisfied myself that enlargement of the prostate was the exception. I had commenced a series of observations among the living and deceased in-pensioners of Chelsea Hospital, which were interrupted, the rough notes of which I still possess. I can confirm Mr. Messer's statement that the proportion of men in advanced life suffering from the consequences of enlarged prostate is indeed small. In 1600 old men, with an average sick-list of 200, he found not more than 10 under treatment for this disease in Greenwich Hospital when his inquiries were instituted, and a half of these only occasionally. A similar immunity exists at Chelsea Hospital among a still older body of men than in the sister establishment."

Dr. Maclachlan expresses a most unfavourable opinion of some of the plans recommended in books as a means of retarding and removing the enlargement of the prostate; he has no faith in blood-letting either locally or in the vicinity of the part affected—sedative suppositories, setons, &c. "These and like measures," he says, "utterly fail in their intention, and some of them at least are worse than useless." He considers the most likely means of checking the progress of the enlargement to consist in "avoidance of stimulating articles of diet, moderation in the use of alcoholic beverages, or in certain cases their entire abandonment; occasional tepid baths, regular walking exercise, implicit attention to the calls of nature, and an open state of the bowels. The mildest laxatives

only are admissible." Some instructive cases are adduced, in which the officious and careless employment of instruments was followed by urgent strangury or complete retention, and in which there appeared every reason to believe these would not otherwise have appeared, or were at least far off. "It is by no means intended," he adds, "by those examples to deter the practitioner from introducing an instrument into the bladder, but only to caution him not to employ it heedlessly, and only when it is necessary to relieve complete or partial retention of urine, since, however carefully passed, the operation appears to be liable to cause mischief in certain states of the enlarged prostate, dependent on temporary attacks of congestion or inflammatory irritation."

The next part treats of affections of the skin; and the concluding portion of the book is devoted to an exposition of the constitutional or blood-diseases which are met with in the aged. We would gladly have dwelt on some of the matters of interest contained in these chapters, but we feel that our limits compel us to bring our remarks to a close, and in doing so we have to express the high sense we feel of the valuable benefit which Dr. Maclachlan has conferred upon medical literature in the work to which we have thus introduced our readers.

NEW EDITIONS.

1. *Dr. J. Moore Neligan's Practical Treatise on the Diseases of the Skin.* Second Edition, Revised and Enlarged. By T. W. BELCHER, M.A., M.D., &c., &c. Dublin: Fannin & Co. 1866. Post 8vo, pp. 526.
2. *Lectures, Chiefly Clinical.* By THOMAS K. CHAMBERS, M.D., &c. London: Churchill. 1864. 8vo, pp. 599.
3. *A Dictionary of Practical Medicine, comprising Special Pathology, the Principles of Therapeutics, the Nature and Treatment of Diseases, Morbid Structures, and the Disorders Specially Incidental to Climates, to Races, to Sex, and to the Epochs of Life; and with an Appendix of Approved Formulæ. The whole forming a Digest of Pathology and Therapeutics.* By JAMES COPLAND, M.D., F.R.S., &c. Abridged by the Author, assisted by JAMES C. COPLAND, M.R.C.S., and M.S.A., and throughout

brought down to the Present State of Medical Science. London: Longmans. 1866. Royal 8vo, pp. 1,537.

4. *Medical Lexicon: a Dictionary of Medical Science, containing a Concise Explanation of the Various Subjects and Terms of Anatomy, Physiology, Hygiene, Therapeutics, Pharmacology, Pharmacy, Surgery, Obstetrics, Medical Jurisprudence, and Dentistry; Notices of Climate, and Mineral Waters; Formulæ for Officinal, Empirical, and Dietetic Preparations; with the Accentuation and Etymology of the Terms, and the French and other Synonyms, so as to constitute a French as well as English Medical Lexicon.* By R. DUNGLISON, M.D., &c. Thoroughly Revised and Very Greatly Augmented. Philadelphia: Blanchard & Lea; London: Trübner. 1865. Royal 8vo, pp. 1,047.
5. *A System of Surgery, Pathological, Diagnostic, Therapeutic, and Operative.* By SAMUEL D. GROSS, M.D., &c. Illustrated by over 1,300 Engravings. Third Edition, Much Enlarged and Carefully Revised. In Two Volumes. Philadelphia: Blanchard & Lea; London: Trübner. Royal 8vo, pp. 1,049–1,087.
6. *Principles of Human Physiology.* By WM. B. CARPENTER, M.D., &c. Sixth Edition. Edited by HENRY POWER, M.B., London, F.R.C.S., &c. London: Churchill. 1864. 8vo, pp. 946.
7. *A Manual of Physiology, including Physiological Anatomy.* By W. B. CARPENTER, M.D., &c. Fourth Edition, with two Plates and 250 Wood Engravings. London: Churchill. 1865. Fcap., pp. 749.
8. *The Microscope and Its Revelations.* By WM. B. CARPENTER, M.D., &c. London: Churchill. 1862. Fcap. 8vo, pp. 792.
9. *A Treatise on the Principles and Practice of Ophthalmic Medicine and Surgery.* By T. WHARTON JONES, F.R.S., &c. Third Edition. Recast, Much Enlarged, and Illustrated by Numerous Additional Engravings. London: Churchill. 1865. Fcap., pp. 806.
10. *Lectures on the German Mineral Waters, and on their Rational Employment; with an Appendix embracing a Short Account of the Principal European Spas and Climatic Health Resorts.* By

SIGISMUND SUTRO, M.D., &c. Second Edition, Carefully Revised and Enlarged. London: Longmans. 1865. 8vo, pp. 419.

11. *Elements of Physics or Natural Philosophy, written for General Use, in Non-technical Language.* By NEIL ARNOTT, M.D., F.R.S. Sixth and Completed Edition. London: Longmans. 1865. 8vo, pp. 726.
12. *The Philosophy of Health, or an Exposition of the Physiological and Sanitary Conditions Conducive to Human Longevity and Happiness.* By SOUTHWOOD SMITH, M.D. Eleventh Edition, Revised and Enlarged. London: Longmans. 1865. 8vo, pp. 395.

WE have again to bring under the observation of our readers a variety of new editions of standard works, and, as on a previous occasion, we shall notice them in a single article.

Neligan on the Diseases of the Skin. Second Edition. Edited by Belcher.—This is another of the works of our predecessor in the Editorship of this Journal that has been republished since his death—a fitting monument to his industry, learning, skill, and reputation. Under the Editorship of our learned contributor, Dr. Belcher, the work has been most fully and accurately brought up to the level of the present day; and, though no opinion, statement, recommendation, or fact recorded by Dr. Neligan has been omitted, the whole has undergone careful revision, while, by the adoption of fuller pages and a slight increase in their number, the volume has been made to contain half as much more matter as the previous edition. The Editor, who, besides having been a pupil of Dr. Neligan, has, as physician to the Dublin Dispensary for Diseases of the Skin, and as the pages of this Journal testify, worked assiduously and carefully at dermatology, avoids, very wisely in our opinion, the system of brackets and foot-notes, and incorporates his new matter with the text, but in such a manner as to indicate very clearly the opinions and statements for which he is himself responsible and those of Dr. Neligan; nor has he been less careful with those of other authors. The additions embrace a very copious and carefully-compiled bibliography, references to the illustrations in Dr. Neligan's *Atlas of Cutaneous Diseases*, together with descriptions of exanthemata and various other diseases not originally

noticed by the author. We can have no hesitation in expressing our opinion that Dr. Belcher has made this one of the most complete and perfect practical treatises extant at the present day on diseases of the skin.

Lectures, Chiefly Clinical. By Thomas K. Chambers, M.D.—This is the second edition of a work, the first edition of which we reviewed in February, 1863. It was then entitled *The Renewal of Life; Clinical Lectures Illustrative of the Restorative System of Medicine, &c.*; and it will be in the recollection of our readers that we then, in common with many of our contemporaries, very freely expressed our condemnation of the title of the book and of certain other parts that we thought calculated rather to attract the non-professional than the professional reader. We have now great satisfaction in observing that the objectionable title and other matters have been removed, and that the work has assumed a strictly scientific tone. Two further editions have, we see by the advertising lists, been published since the one before us, and if where a book has reached a fourth edition the commendation of a reviewer could be of any value to it, we would very cordially endorse the verdict our profession have already pronounced, and recommend the book in its present improved condition to all our readers. Dr. Chambers carries his readers to the bedside, and there illustrates and tests his theories. He is evidently a deep thinker and a scientific physician, who seeks to treat disease on sound principles and not by routine or dogma.

Copland's Dictionary of Practical Medicine.—We have here Copland's *Dictionary*, originally published in four volumes, condensed into one, certainly not a small one, but still one volume. The title-page, which we have copied above, is so full that even if the work were an unknown one, it would leave little for a reviewer to say as to its contents. The abridgment has been executed by the author, with the assistance of his son, which is a sufficient guarantee that it has been carefully done. In order that all really practical subjects should be retained, and not so abridged as to impair their value, several articles appertaining rather to the subordinate sciences and to general pathology, the bibliography and references have been excluded; and part of the space thus gained has been occupied with notices of the more important contributions to medical science since the publication of the original edition.

Dunghlison's Medical Dictionary.—This work, unlike that of Dr. Copland, which we have just noticed, is more of a lexicon than an encyclopedia of medical science; and yet not a mere lexicon, for, under all the principal terms, a well condensed summary is given of the subject to which the term refers. We have long been in the habit of using an earlier edition of this lexicon, and have found it most useful and correct. The present edition has been enlarged and revised.

Gross' Surgery.—In November, 1859, and in February, 1863, we commended very highly the first and second editions of this work, and we have now only to renew our former praises.

Carpenter's Human Physiology; Manual of Physiology; Microscope, and its Revelations.—Dr. Carpenter announces in the preface that, finding his time much occupied with his official engagements, and wishing to devote what leisure he had to original investigations, he resigned the preparation of the sixth edition of his *Human Physiology* to the care of Dr. Power, “a gentleman in whose ability and conscientious zeal he felt great confidence;” and that with the exception of the first and second chapters, which were prepared before he decided on taking this step, he holds himself exempt from responsibility in regard to either the scientific character or the literary execution of the present issue, in which, moreover, he has no pecuniary interest whatever.

We have examined this volume with considerable care, and have no hesitation in saying Dr. Power has admirably succeeded in accommodating it to the present state of physiological science. In every chapter we see the latest results of original observations carefully analyzed and clearly stated; and by omitting the sections on the functions of the cerebrum, which belong rather to psychology than to physiology, and which Dr. Carpenter holds out a hope that he will elaborate into a manual of psychology, space has been gained, which has been devoted to subjects more truly physiological, consequently, though much new matter has been added to the volume, its bulk has not been increased. We have no doubt *Carpenter's Human Physiology*, edited by Power, will maintain the position so long and so worthily held by the successive editions published under the author's own supervision. With regard to the other volumes bearing Dr. Carpenter's name, it is only necessary to say that these editions have come out under his own superintendence.

Wharton Jones on Ophthalmic Medicine and Surgery.—In no department has the science of medicine made more progress recently than in that of which this volume treats; but we have, during the last year, devoted so much space to optics and ophthalmic surgery that we can only announce the appearance of the present edition of Wharton Jones' well-known treatise, and acknowledge the general accuracy of his assertion that he has "endeavoured to make it as complete an exposition as possible of the subject in its present advanced state."

Sutro on German Mineral Waters.—This work has grown to its present dimensions from a series of lectures delivered at the Hunterian School of Medicine, and published in the pages of one of our contemporaries, where they met with such a reception as to induce the author to collect them into a volume and publish them separately. A new issue has now been again called for, and Dr. Sutro has added, in an Appendix, a short but comprehensive account of all the important health resorts not treated of in the former edition. We suppose it is impossible for any one, especially a German, to write a book about mineral waters without attributing to them mysterious properties, not to be explained by the ordinary laws of physics; and we are not, therefore, surprised at Dr. Sutro "assuming that the caloric produced by volcanic processes exerts a different influence on the human system than that which has been artificially produced." However, notwithstanding these heresies about "the peculiarity of the *inherent* heat of the springs," and such like, we believe this to be a very excellent manual on the subject of which it treats, and one the physician will find most convenient for reference, and useful in his every day practice.

Arnott's Physics and *Southwood Smith's Philosophy of Health.*—These works are intended for the use of the non-medical world, and we believe no greater service could be done, both to the profession and to society at large, than the cultivation of a taste for information such as these admirable volumes are well fitted to afford.

1. *Lectures on the Pathology and Treatment of Lateral and other forms of Curvature of the Spine.* By WILLIAM ADAMS, F.R.C.S. London: Churchill & Sons. 1865. 8vo, pp. 334.
2. *Curvatures of the Spine, their Causes, Symptoms, Pathology, and Treatment.* By BERNARD E. BRODHURST, F.R.C.S. London: Churchill & Sons. 1864. Royal 8vo, p.p. 93.

TREATISES on spinal curvatures and general deformities have in so many instances been but the advertising media of charlatans and empirics that we have long had an almost instinctive dislike to the very names of them. The treatises by Messrs. Adams and Brodhurst, which we have now to introduce to our readers, are, however, of a very different class. They are the productions of men who have carefully worked out their subject, and honestly laid the results before their brethren, and we propose to analyze them somewhat fully, so as to show the present state of our knowledge on the subjects of which they treat.

The opportunities presented to students for studying these affections are very few, so that most of us have been in the same position as Sir Benjamin Brodie, who stated in one of his lectures delivered in 1846, that when he first became engaged in considerable private practice, and cases of curvature of the spine were presented to his observation, he was in doubt as to their nature and treatment. "I knew nothing of them," he says, "from my own experience, and I had learned nothing of them from my teaching. I felt that in this respect my education had been imperfect, and it took me no small trouble to supply the deficiency." It must be placed to the credit of special hospitals, against which so much has been said and written, that they have sent forth works so well fitted as those before us to supply this deficiency in medical education.

Spinal curvatures to a slight extent are very common, and when they do not exceed certain limits are of very trivial importance; but extreme uncertainty must always exist as to their progress; for though some may undergo spontaneous arrest at an early period, in other cases the curvature increases so as not only to produce deformity, but to seriously injure the general health by producing local pain, or by interfering with the action of the heart and lungs, or digestive organs.

Some controversy has taken place as to whether there is any normal lateral curve in the spine. As is well known, the majority

of our anatomical writers describe a normal lateral curve in the dorsal region, convex towards the right side; and some believe there is another curve in the opposite direction, in the lumbar region, which Bühring names the *hepatic curve*, to distinguish it from the other, which he calls the *cardiac*. Mr. Adams "entirely concurs with Dr. Little in doubting the existence of lateral curves as a normal condition of the spine," and states that during the twelve years from 1842 to 1854, that he acted as Demonstrator of Morbid Anatomy at St. Thomas's Hospital, he habitually examined the spine after eviscerating the bodies, and remarked the great rarity, rather than the frequency of any lateral curve. Mr. Brodhurst says he has himself seen the measurements made by Bühring, and can bear testimony to their accuracy; and we must confess that strong as Mr. Adams' statement is, we are not prepared to give up our belief in the existence of a normal lateral curve in the dorsal region; but the fact that the existence of such a curve is doubted ought always to be sufficient to put us on our guard, when consulted about a slight curve in young subjects, even in this region, and to teach us to watch carefully lest it may increase and become a serious deformity. The antero-posterior curve, arising from caries of the vertebrae, is not taken into consideration by either of our authors, but only those forms of curvature to which the terms *lordosis* for the anterior curvature, *cyphosis* for the posterior, and *scoliosis* for the lateral, have been applied since the days of Hippocrates.

Lordosis, or anterior curvature, chiefly affects the lumbar region, being an exaggeration of the normal curve found there, but it may also occur in the cervical, and even in the dorsal regions; and Mr. Adams devotes separate sections to the consideration of it in these situations. The appearances produced by it in the lumbar region are well displayed in the illustrations given by both authors. The nates are raised, the lumbar region is rendered remarkably hollow, the dorsal is rounded, and the head is thrown back, the abdomen is unusually prominent, and the stature stunted. This deformity is often hereditary, affecting several members of the same family. It occurs in rickets, of which it may be the only sign, or it may be only one of the signs of a severe and general form of this affection. The softened pelvis becomes distorted from the superincumbent weight, the sacrum becomes horizontal, and the lumbar curve is increased to maintain the equilibrium. In some cases in which the projection anteriorly has been very great, it has been attributed to a dislocation of the lumbar vertebrae. Such a dislocation is,

however, impossible. The deformity may be so great as to prevent natural parturition, and even render the Cesarean section necessary. Whatever tends to cause obliquity of the pelvis may produce lordosis. Both Mr. Adams and Mr. Brodhurst refer to cases of congenital dislocation of the hip as causing it; and Mr. Adams remarks that where both hips are dislocated, the true nature of the case is generally overlooked. Unreduced non-congenital dislocation will also produce it, as well as ankylosis of the hip joint, with the thigh flexed, or indeed anything causing one leg to be shorter than the other. It may also coexist with caries of the lumbar vertebrae in an early stage, or be consecutive to arrest of destructive disease, and the occurrence of angular curvature in the lower dorsal region. Mr. Brodhurst describes a case in which a very different cause produced the curve—the muscles of the abdomen had undergone fatty degeneration, and the whole viscera hung suspended in a huge tumid paunch, the weight of which had in part caused the distortion which was one of the most remarkable he had ever witnessed. The fatty degeneration had affected many of the muscles of the back and of other regions, adding to the effect. The gravid uterus sometimes produces lordosis by its weight, and the carrying of heavy weights round the neck and shoulders may have a similar effect.

A consideration of the causes of this form of curvature shows that the treatment resolves itself chiefly into that of the condition tending to produce the curve; if there be constitutional debility or rickets, treatment must be directed to the removal of these conditions. Both authors urge that in the case of rickets the horizontal position, prone or supine, should be maintained, the children being induced to exercise themselves with weights, pulleys, and ropes, or elastic bands properly arranged, as suggested by Mr. Brodhurst, who, however, permits any exercises when the distortion is very slight only; when it is severe he says it should be entirely prohibited. The recumbent position is the most powerful means we possess for removing lordosis; but in some cases it may not be practicable to maintain it constantly, and then Mr. Brodhurst applies a light portable instrument, which receives the weight of the body and transmits it to the pelvis, relieving the spinal column; but he finds this a very imperfect substitute for the recumbent position. In speaking of lordosis in connexion with caries, Mr. Adams takes occasion to say that, contrary to the directions given in all surgical works, that the patient should be kept lying during the progress of the caries, he has for more than twelve years adopted

the practice of applying mechanical support to the spine in these cases, by means of either a leather or steel apparatus, according to age, and allowing the patient to walk about when so inclined, which he finds to promote the proper constitutional treatment, and to be applicable in all stages of the caries.

Cyphosis, or posterior curvature, is one of the most common forms of spinal distortion, and may occur in infancy, youth, or old age. The term has been applied to the angular curvature arising from caries; but our authors, as has already been stated, do not include this in their description. The disease is one essentially of debility, whether in infancy, youth, or old age; it may arise from rachitis, muscular rheumatism, or partial paralysis, as well as from the habitual use of a stooped attitude, as in certain occupations. Such cases are best treated by a combination of local support to the spine and constitutional treatment. Where the debility is great in children, the horizontal position must be maintained till they have the power to sit or stand erect. In slighter cases a splint of leather or gutta-percha may be applied to the back. A weighty apparatus is liable to do more harm than good. After childhood a portable apparatus may be used with advantage, and much benefit may be derived from the judicious use of gymnastic exercises.

We now enter on the consideration of scoliosis or lateral curvature, and the great importance of this form may be estimated from the fact that both Mr. Adams and Mr. Brodhurst devote more than two-thirds of their volumes to its description. Mr. Brodhurst divides cases of this kind into two classes, *incipient curvature*, or that condition which is removable in the horizontal posture by slight lateral pressure, and *confirmed lateral curvature*, which requires long-continued mechanical means to effect the same object. Mr. Adams makes but one class; he defines lateral curvature as a deformity or distortion of the spine in which the bodies of the vertebræ deviate laterally in a horizontal direction, with or without a corresponding deviation of the apices of the spinous processes. He undertakes to demonstrate that in many severe cases of scoliosis the spinal column presents on its anterior aspect a large sigmoid curve, whilst posteriorly very little deviation exists in the perpendicular line of the apices of the spinous processes; and that in slight cases, in which an internal curvature affecting the bodies of the vertebræ essentially exists, there may be no external curvature or lateral deviation of the apices of the spinous processes. Mr. Adams asserts that it is anatomically impossible for any distortion of the spine,

such as lateral curvature, however slight, to exist without being accompanied with structural alterations proportionate to the extent and duration of the curvature; but he admits the existence of a large class of cases simulating lateral curvature, and which he calls *weak spines*, or *cases of threatened lateral curvature*, which may terminate in confirmed lateral curvature if not prevented by proper treatment. These cases are, he says, described by many authors as the first stage, and not only magnified into undue importance but treated by spinal instruments, when really they only require gymnastic and constitutional treatment. We apprehend most of our readers, however they may agree with Mr. Adams as to the mode of treatment of these cases, will concur with those authors who regard these *weak spines* as really cases of lateral curvature in the first or incipient stage.

The symptoms of lateral curvature are very fully described by Mr. Adams, and more summarily by Mr. Brodhurst; both authors give illustrations showing the peculiarities of the curve as presented in different patients; but it is unnecessary to dwell on these here, and we pass to the consideration of the morbid anatomy of this form of curve; and here we will allow Mr. Adams to speak:—

“In the description just given of the *external characters* of lateral curvature in its different forms, I have already indicated the fact to which I would now more especially direct your attention—viz., that in lateral curvature the spinal column does not yield in a purely lateral direction, as a flexible column would bend, but presents the appearance of a spiral twist, owing to the bodies of the vertebræ turning round in a direction of *horizontal rotation*, so that their anterior surfaces are directed laterally along the convexity of the curvature. In a severe case this rotation commonly extends to a quarter of a circle in the centre of the curve, and diminishes from this point to the two extremities, so that the vertebræ unequally turned upon themselves, cease to correspond in their natural relations to each other, as well described by Bouvier. In a case of extreme severity, this rotation will sometimes extend even to half a circle, so that in the centre of the curve the anterior surfaces of the bodies of the vertebræ have a directly lateral aspect; in such cases the angles of the ribs project posteriorly, and are bent sharply upon themselves, extreme deformity of the chest of course resulting.

“This deviation of the bodies of the vertebræ does not necessarily correspond to, nor is it indicated by, any lateral deviation of the apices of the spinous processes. It may exist to a considerable extent—the rotation extending to a quarter of a circle in the centre of the curve—without any, or at least with very slight, lateral deviation of the apices

of the spinous processes, as shown by the preparation from which the drawings in Plates III., IV., and V. were taken."

Mr. Adams gives a diagram illustrating the manner in which the vertebræ rotate on one another, a morbid change first observed by Dr. Dod, in 1824, but not fully understood; and Mr. Adams claims to have shown, for the first time, the existence of internal without any external curvature or lateral deviation of the apices of the spinous processes, either in severe or slight cases, and the means of detecting this deviation, and gives the following description of the effects on the figure:—

"If it be rare to find the extreme degree of internal, coexisting with an absence of external curvature, as in the case above described, it is undoubtedly true that internal curvature frequently exists to a slight extent without any external lateral deviation of the apices of the spinous processes, as well exhibited in Plate II., and the class of cases in which this condition will be found is where a prominence, or a *growing out*, as it is called, of one shoulder is said to exist without any spinal curvature. Such cases are frequently the cause of great anxiety to parents, but are dismissed by surgeons with the comforting assurance *that there is no curvature of the spine*, whereas the prominence of the shoulder, *i.e.*, the posterior projection of the scapula and the angles of the ribs, might most certainly be relied upon as indicating the existence of internal curvature, or lateral deviation of the bodies of the vertebræ. The prominence, or *growing out*, of the shoulder ought therefore to be regarded as indicating the existence of internal curvature of the spine, rather than an unimportant external appearance depending, as it is generally said, upon enlargement of the muscles caused by excessive use of the right arm.

"A little consideration, and the examination of specimens preserved in museums, will convince you that the spiral twist which the spine undergoes in lateral curvature, and which is due essentially to a *horizontal rotation* movement of the bodies of the vertebræ, must necessarily produce a *posterior projection of the angles of the ribs*, on the side of the convexity of the curve, in the dorsal region; and a *posterior projection of the transverse processes* on the side of the convexity of the curve, in the lumbar region, in a degree more proportionate to the extent of deviation of the bodies of the vertebræ; whilst in the concavity of the curve, the angles of the ribs in the dorsal, and the transverse processes in the lumbar region are depressed or sunk inwards to a corresponding extent, and these conditions may be more certainly relied upon than the spinous processes, as indicating the existence of internal curvature or lateral deviation of the bodies of the vertebræ."

The structural changes in these cases affect the inter-vertebral substance and the bodies of the vertebræ themselves, which become thin and wedge-shaped in the concavity of the curves, but the articulating processes are still more remarkably altered. Even at the commencement of the disease they become altered in direction and aspect, and in severe cases, of long standing, they may present an irregular enlargement and nodulated appearance round the margins of the articular facets, resembling the condition found in chronic rheumatic arthritis. The transverse processes, also, not only have their direction altered, but may have their figure changed, probably from the effect of altered muscular action, becoming attenuated and projecting backwards in a curved or horn-like form. Mr. Adams believes that these changes occur in all classes of cases; and, contrary to the opinion of some writers, that there is no special connexion with rickets, or special liability to lateral curvature in consequence of the abnormal condition of the bones in cases of rickets; but that when curvature occurs in rickets it is at a late period, and in consequence of a difference in the length of the legs from rachitic deformity. On this point Mr. Brodhurst differs in opinion, and describes a special rachitic scoliosis in which the bodies of the vertebræ become softened, and yield, consequently, to pressure.

In the following passage we have the key to Mr. Adams' views as to the etiology, and mode of production, of lateral curvature; it is taken from the beginning of the sixth lecture, which, with the two following lectures, is devoted to an elaborate exposition of his views, and discussion of those of other writers:—

“I know of no affection, the treatment of which should be more directly influenced by a consideration of the causes which produce it, than lateral curvature of the spine, and therefore we cannot too closely analyse the precise nature, and *modus operandi* of the various causes which are said to produce this form of spinal curvature. Generally in books on lateral curvature, we find the causes traced either to certain conditions of the muscular system, or to certain occupations associated with these conditions; thus in the most recent of these books, we find the causes arranged under the several heads of: 1st. muscular and ligamentous debility; 2nd. muscular hypertrophy; 3rd. muscular atrophy; and 4th. spasm of the muscles, and though other causes, such as obliquity of the pelvis, rickets, and some organic affections are admitted as causes, the cases are generally assigned to the muscular conditions above adverted to. In another book, we find only one class of cases described, viz.: those depending upon muscular debility, the other forms of this affection being

merely enumerated. We cannot, therefore, be surprised to find only one method of treatment, such as a spinal instrument of the author's invention, recommended for all cases. Now, it appears to me, that a careful study of this affection must necessarily lead to the conviction that lateral curvature of the spine depends very largely upon certain *constitutional conditions*, such as an *hereditary tendency* to this affection, which may be traced probably in half the cases which come under our notice; a *strumous diathesis* associated with which the worst forms of curvature, often erroneously classed as cases of rickets, are frequently seen; and *constitutional debility* either existing from infancy or induced by febrile affections, &c.

"Co-existing with these constitutional conditions we can also in most instances, in which the spinal curvature is produced after 10 or 12 years of age, but not in those of an earlier date, trace certain *local causes* acting mechanically so as to disturb the equilibrium of the spinal column; and upon a careful consideration of the relative causative influence of these constitutional and local causes, in any given case, I believe it is alone possible to determine the principles of treatment applicable to such a case.

"It appears to me, therefore, that the causes of lateral curvature may very properly be arranged in two classes—viz., the predisposing, and the proximate causes.

"1st. *Predisposing causes.* These are essentially constitutional, either hereditary or acquired, such as hereditary predisposition; strumous diathesis; constitutional debility; rickets; induced or temporary muscular debility.

"2nd. *Proximate causes.* These are essentially local or such as act mechanically upon the spinal column to disturb its equilibrium, such as:
a. The long continuance of certain bad positions, such as the habit of standing on one leg; the sitting position during education, especially for writing and drawing purposes; sitting cross-legged and in a stooping position, as some young ladies do for several hours a day, when fond of reading; excessive horse exercise, to which delicate girls are frequently subjected with the object of improving the general health, but often with the result of producing a curvature of the spine with rotation in the lumbar region. *b.* certain occupations, such as needlework, envelope and book folding, flower making, &c.; also nursing children, or carrying weights; ironing, and any other occupation which may render necessary the long continuance of any position which would disturb the equilibrium of the spinal column. *c.* inequality in the length of the legs from any cause; the use of a wooden leg, &c.; and *d.* certain diseases of the chest, such as empyema, and some other affections of the thoracic and abdominal organs.

"The *modus operandi* of all these proximate causes is essentially similar,

the resulting curvature of the spine being produced by the *long continued irregular distribution of weight*, resulting from any position which, for a sufficient length of time, disturbs the equilibrium of the spinal column, and throws the weight continuously in the same direction.

“The immediate cause, therefore, in all cases is essentially mechanical, and I have already demonstrated in the preceding lectures that the structural changes produced are merely the effects of mechanical pressure.

“In explaining the origin and mode of production of any case of lateral curvature, we must first endeavour to ascertain the existence of any of the predisposing causes, and then recognise the purely mechanical cause, *i.e.*, the irregular distribution of weight, as immediately and directly producing the structural changes which render the curvature fixed, persistent, or confirmed as it is frequently called; it being considered as proved that no persistent curvature, however slight, can exist unaccompanied by such structural changes.”

In discussing the various theories that have been proposed as to the cause of lateral curvature, Mr. Adams devotes special attention to the *muscular theory*, as he calls it. M. Guerin is the great advocate of this theory, maintaining that the lateral curvature depends primarily and essentially upon muscular retraction of an active character, caused by some abnormal condition of the nervous centres. On this theory the proposal to treat these cases by subcutaneous section of the muscles is founded; a method so ably and forcibly advocated by M. Guerin, as to have very favourably impressed both the profession and the public in its favour. In one case Mr. Adams was induced to adopt the practice, but subsequent experience convinced him of its impropriety; and he now shows, both by *post mortem* examinations, and a critical examination of the theory, that it is altogether fallacious. It appears to us that in controverting the muscular and other theories Mr. Adams has expressed himself so that his readers may overlook what he says on the constitutional causes, and regard the mechanical causes as the sole agencies in the production of the curvature. On this point, though equally regarding the mechanical as the exciting causes, Mr. Brodhurst expresses himself more in accordance with what we consider the relative importance of the two sets of causes:—

“The causes of scoliosis are various. They are general and local debility, rickets, thoracic disease, obliquity of the pelvis, &c.

“*Debility* is, without doubt, the most common cause of scoliosis. It shows itself in various ways. Thus, muscular debility during convalescence,

and weakness of the ligaments during too rapid growth, give rise very frequently to this common affection.

“Let us take, for instance, the case of a child whose health has been rendered delicate from whatever cause. If it be treated in the same manner as a stronger companion, some irregularity of form will probably develope itself; but the particular form of distortion will depend mainly on the habits of the child. For instance, if the child stand or walk much, it is probable that the internal lateral ligament of the knee-joint will yield, and give rise to genu valgum; and this distortion will probably be followed by yielding of the ligaments of the ankle-joint, and of those in the sole of the foot—valgus. Let us, suppose, moreover, that in consequence of resting too much on one leg, as is very common with such children, the ligaments of that extremity have yielded more than those of the other. Virtually, that leg is shorter than the other; and, consequently, both feet resting on the ground, the pelvis necessarily becomes oblique to accommodate itself to the difference in length of the extremities. But an oblique pelvis must give rise to spinal curvature, as will be explained farther on.

“Those who are feeble, whether from overgrowth or during convalescence, constantly change their position while standing; so that they rest for a short time only in one position and then change it for another—from both feet to one foot, for instance, and back again. And it is found that it is habitually the right foot in some cases, and in others the left foot, which has, in this way, to bear very frequently almost the entire weight of the body, until, at length, it becomes more natural to stand on one leg only than fairly on both. It becomes so natural to do this, that it is done unconsciously; and until it is pointed out, the patient is frequently not aware of the habit. This occurred, and it was frequently pointed out, in the case from which Figure 9 was taken.

“Debility alone does not usually give rise to lateral curvature of the spine; but there must be superadded bad habits of standing or sitting which shall occasion obliquity of the pelvis and a primary lumbar curve. These habits to which I have alluded depend on debility, however; and therefore it is right to speak of debility as the cause of these distortions. But without obliquity of the pelvis, and therefore without bad habits of sitting or standing having been formed, lateral curvature of the spine may be occasioned by general debility. It may commence as a dorsal curve, as was the case with the patient from whom the following figure (Figure 10) was taken. This is a form of curvature which commonly begins during convalescence; and commencing as a dorsal curve, it shows that it does not depend primarily on obliquity of the pelvis, nor on any affection of the lower extremities.

“Every distortion is produced by a special cause. It need scarcely be said that while the cause of curvature remains, the curve itself cannot be

removed; or if it should be temporarily removed, it will certainly recur, inasmuch as the cause which first occasioned it still exists. 'This question of cause is one of the utmost importance, therefore, in the treatment of spinal curvature. It is greatly neglected, however, and misunderstood, so that with many the treatment of spinal curvature is simply to apply a spinal instrument, without reference to the cause of curvature.'

Mr. Brodhurst further speaks of scoliosis as hereditary in some cases, where he attributes it to rachitis, or malformation, or nervous irritation. Paralysis of one side of the trunk will, he shows, allow the healthy muscles to draw the spine away from the mesial line, and so cause a curvature, and also the habitual carrying of weights on one arm. The wearing of stays Mr. Brodhurst looks on as a predisposing cause of considerable importance; the muscles of the back, he says, become atrophied, allowing the causes which tend to produce curvature to act with increased force. Mr. Adams, however, argues, that in the great majority of cases the curvature forms at an early age, and before the practice of tight lacing is adopted, and he altogether rejects this as one of the causes of curvature.

The treatment of lateral curvature must be both constitutional and local, and directed with a view to the removal of the causes of the affection. Debility must be corrected by the adoption of proper constitutional and hygienic treatment, and local causes must be removed. If the curvature be caused by obliquity of the pelvis, from one of the legs being either actually shorter, as from atrophy, or relatively shorter, as from inflection of the knee, it will be useless to attempt to remove the curvature without first correcting the disproportion of the legs; and when this is duly done, the curvature will in a great measure disappear, without any treatment specially directed to it. Mr. Adams enumerates five systems of local treatment as having been advocated, and reported at times as in every way successful, and enters into an examination of their merits:—

- "1st. The system of *complete recumbency*.
- "2nd. The system of *muscular exercise, or gymnastics*.
- "3rd. The system of *cutting the spinal muscles*.
- "4th. The system of *mechanical extension*.
- "5th. The system of *mechanical treatment by support and pressure*, applied by means of spinal instruments."

Of these, Mr. Adams regards recumbency as a curative means, but he rejects it as a system to be relied upon exclusively. He

recommends it in every case during the period of growth in conjunction with such other means as may be thought advisable; in some instances with gymnastic exercises, in others with mechanical support, and occasionally, also, in conjunction with both these methods of treatment. In ordinary cases he recommends recumbency during four or six hours a day, which can be borne with comfort, and without disadvantage to the general health, he says, by using a reclining chair that he gives a figure of. Gymnastics and muscular exercise he also thinks capable of being made useful, but he does not at all admit that they can cure a curvature, though in threatened cases they form the best means of prevention.

Cutting the muscles he entirely disapproves of, as well as of the system of extension in either the horizontal or erect posture. The system of mechanical treatment by means of spinal instruments may, he says, be described as the English orthopedic treatment of the present day. It has been adopted and advocated, he says, by the majority of English authorities of late years, to the exclusion, it may be feared injuriously, of all other methods of treatment:—

“The instruments now in use vary considerably in their principles of construction, as well as in the mechanical details; and the kind of apparatus to be applied in any particular case must depend upon the form and situation of the curve, and also upon the age of the patient.

“There can be no doubt that the greatest advance which has taken place in modern times in the treatment of lateral curvature of the spine, has been made through the *system of mechanical treatment*, by means of spinal instruments; but as might have been anticipated from the novelty and importance of the system, too much has been expected from it, and also too much claimed for it by its principal advocates.

“Compared with other systems, there can be no question as to the superiority of the mechanical treatment, and if one plan alone were to be adopted, this would be most entitled to our confidence; but the evidence which I have laid before you of the constitutional, as well as the local causes which contribute to the production of spinal curvature; and the nature of the structural changes which occur even in slight cases of lateral curvature, must prevent our placing sole and implicit confidence in the power of any spinal instrument to cure the curvature.

“I have no hesitation in stating, that it is impossible to straighten a curved spine by any mechanical means, in the same manner and upon the same principle as we can straighten a bent knee, ankle, or any other movable articulation; but in combination with other means, such as partial recumbency, and in some slight cases with muscular exercises, it is quite possible to cure a curvature of the spine when slight and of recent

formation in young persons ; and it is certainly within our power most effectually to arrest the progress of curvature when more severe."

The remaining chapters in Mr. Adams' book are devoted to the description in detail of the treatment of lateral curvature. The cases are divided into classes according as constitutional or local causes predominate in their production, and while the constitutional treatment is placed in the first rank of importance, the construction and mode of application of the instruments that may be used as accessories to it are clearly explained and illustrated by diagrams.

In conclusion, we would reiterate our opinion that both of these works are of very great value. Mr. Adams gives all the details of the anatomy, pathology, and treatment, Mr. Brodhurst, without entering into these so fully, enunciates his principles clearly. We believe the reader cannot fail to derive from the study of either one or other of the works enlarged and correct views as to the nature and treatment of spinal curvatures.

An Essay, Historical and Critical, on the Mechanism of Parturition.

By WILLIAM LEISHMAN, M.D., &c., &c. London: Churchill.
1864. 8vo., pp. 129.

A CORRECT knowledge of the mechanism whereby the human fetus is transmitted through the pelvic canal, during labour at full term, was, doubtless, essential towards the perfection of the practice of obstetrics.

It is generally considered at the present day that our information regarding this process is pretty accurate. The first step towards its elucidation proceeded from Ould, of Dublin; and the theory was gradually more fully developed by various writers, till at length the great publication of Nægèlé, of Heidelberg, appeared, which seemed to have thrown on the subject all the light that was necessary.

The results of Nægèlé's publication were wonderful, his doctrines having been the means of preventing much suffering, and decreasing to a considerable extent fetal mortality—results especially appreciable, when the issues of former practice are compared with those of one based upon his views.

To assert, however, that in the most minute particulars of this process our views, even now, are perfectly correct, from whatever

source derived, theoretical or practical, would be stating too much. But we do not consider that the knowledge of such extreme minutiae is practically essential. We feel convinced, moreover, that in no single instance can it be satisfactorily demonstrated, that a fetal head, say in the first position, follows, in the most minute particulars, the directions, &c., taken by any previous similar presentation and position.

The broad principles of the mode of the head's progress in all first positions are decidedly identical, and that answers all practical purposes. The same may be said concerning all other positions and presentations. The extreme niceties of the mechanism of parturition are, we fear, impossible of appreciation with sufficient accuracy to enable us to arrive at *absolute* correctness, and fortunately for practice such perfection is unnecessary, however interesting and curious it may be to attempt to study the subject thus exactly.

Dr. Leishman has devoted himself to the consideration of this subject with extreme assiduity, and the present volume is the result of his labours.

In the Preface he informs us that "too much has been taken for granted" concerning "the mechanism of labour," and that "there is yet much more to learn." Most true; and we do not know to what branch of medical science these assertions are not equally applicable. Taking "too much for granted" was one occasion of the tardy progress of the Irish school of midwifery. It is but of comparatively recent date, when the dogmatic assertions of the leaders in this branch of medicine were invariably "taken for granted;" so that few attempted to turn from the beaten track because of this blind belief; while those few who had the will to deviate from settled routine were restrained by another element against progress—fear. To have even *attempted* to *examine* into the truth of certain dogmas would have given offence; but to have *ignored* those dogmas, and to have set up a new theory, would, possibly, have caused the ruin of the rash offender. Thus, in one word, enquiry was "burked." These trammels have latterly been broken. The *literæ prescriptæ* of the old obstetric legislators of the Dublin school have been examined and discussed upon with a freedom and boldness that would formerly have created amazement. And what has been the result? Why, many of their most cherished aphorisms, formerly "taken for granted" as correct, have been found quite the contrary. Not so very long since, the Dublin

school of midwifery was decidedly behind that of Edinburgh; and it is really of comparatively recent date that she has arrived at her present eminent position, so suitable to her facilities and resources—one by no means inferior to that of any other school in the world. The alumni of the Dublin school no longer take things “for granted.” They have by degrees gathered pluck enough to discuss theories, and overturn them if necessary, no matter by whom propounded. A spirit of free inquiry has now usurped the place of servile obsequiousness. “The good old days” (?) are gone.

The position of senior and junior obstetricians in their relations the one towards the other, some years ago, in this city, reminds us forcibly of a scene narrated by Dickens in one of his stories. It is certainly familiar to all our readers, but its appositeness must be our excuse for inserting it:—“Mr. Willet, after having flattened his fat nose against the cold glass (window), and shading his eyes that his sight might not be affected by the ruddy glow of the fire, looked around. Then he walked slowly back to his old seat in the chimney corner, &c., &c., said, looking round on his guests, ‘It’ll clear at eleven o’clock. No sooner and no later. Not before and not arterwards.’ ‘How do you make that out,’ said a little man in the opposite corner; ‘the moon is past the full, and she rises at nine?’ John looked sedately and solemnly at his questioner, until he had brought his mind to bear upon the whole of his observation, and then made answer, *in a tone which seemed to imply that the moon was peculiarly his business, and nobody else’s*—‘Never you mind about the moon. Don’t you trouble yourself about her. You let the moon alone, and I’ll let you alone.’ ‘No offence, I hope,’ said the little man. Again John waited leisurely until the observation had thoroughly penetrated his brain, and then replying, ‘No offence *as yet,*’ &c., &c.” John Willet is a fair representative of the old obstetric ruler of that day, and the little man his junior.

We admire free inquiry and independent opinion. No matter how much we may respect the conceptions of a practitioner or the doctrines of a teacher, it is our duty, so far as we are capable, to examine for ourselves whether the practice of the one or the teachings of the other be correct. Therefore, although we may have been prepossessed in favour of doctrines, coming from so high a source as the elder Nægèlé, concerning the mechanism of parturition, yet we have always considered it our duty, to endeavour in our practice, whenever an opportunity offered, to discover if his doctrines were correct. We have put him to the test in every case favourable

for that purpose, and the conclusion always arrived at was, that his teachings bore the stamp of truth.

We think it praiseworthy for every one to put Nægèlé's doctrines to the test of practical experience, and endeavour to find out if there may not yet be some weak points and inaccuracies amongst them. This is what Dr. Leishman aims at, while he "endeavours, as far as he can, to throw such light on his (Nægèlé's) doctrines as have been misunderstood or controverted by late writers."

The volume contains five most interesting chapters, and may be said to consist of two parts—one historical, the other practical. The first two chapters include the history of obstetric medicine, more especially as relates to the mechanism of labour; and we are brought down from the opinions of Hippocrates to those of Halahan! These chapters are replete with information, agreeably put together, very concise, and most worthy of the student's perusal.

With respect to the mechanism of labour, our author takes as his theme the three great points mentioned by Nægèlé, in his doctrine concerning the manner in which the fetal head passes through the pelvic canal; and to the consideration of these he chiefly confines himself.

We shall briefly state what they are.

According to Nægèlé, the third position, *i.e.*, the left bregmatocotyloid, is next in frequency to the first position, *i.e.*, the left occipitocotyloid. To make the matter plainer, that position in which the anterior fontanelle occupies a place behind the left cotyloid cavity, and the posterior fontanelle corresponds to the right sacro-iliac synchondrosis (the third), is next in frequency to that position in which the posterior fontanelle is just behind the left cotyloid cavity, and the anterior fontanelle corresponds to the right iliac synchondrosis (the first). That the ratio of the former is to that of the latter as two to five; and that the third position (*i.e.*, where the anterior fontanelle is opposite to the left cotyloid cavity, and the posterior opposite the right sacro-iliac synchondrosis) is almost invariably changed into the second. So that the second position (*i.e.*, where the posterior fontanelle corresponds to the back of right acetabulum or cotyloid cavity, and the anterior fontanelle occupies a position opposite the left sacro-iliac synchondrosis, or right occipito cotyloid) is changed into a very subordinate position as regards its importance—being the least frequent of the four vertex positions, and met with more rarely than face cases. Thus,

according to Nægèlé, the first and third positions are virtually the ordinary forms of vertex presentation; the second (right occipito-cotyloid) being merely a phase of the third (left bregmato-cotyloid) in its progress; and similarly, the fourth (*i.e.*, where the anterior fontanelle is opposite the back of the right acetabulum, and the posterior frontanelle opposite the left sacro-iliac synchondrosis, or right bregmato-cotyloid) is an early stage of the first position. In short, that virtually there are only two positions. The next point is, that from Nægèlé's descriptions no room for doubt was left as to his meaning, that there existed a lateral flexion of the fetal head; in other words, an approximation of the ear to the corresponding shoulder. That in the first position there exists an approximation of the left ear to the shoulder of the same side, is "a point," says our author, "though denied by some eminent obstetricians, almost universally taught in the British schools."

The third great point taught by Nægèlé is, that not only does the obliquity of Saxthorpe and Solayres, viz., that by which the opposing diameter of the fetal head is directed from one sacro-iliac synchondrosis to the opposite foramen ovale (or back of the acetabulum), obtain at the brim, but also at the outlet, "a fact which has been denied by more than one more modern writer than Nægèlé; so that the head does not pass with the long diameter in the antero-posterior direction, but with the sagittal suture directed obliquely across the long diameter of the external opening of the vulva." "I need not add," proceeds the author, "that although there is not one of the doctrines of Nægèlé that has not been questioned by some one or other, his views, as stated above, were almost universally adopted, and may with perfect propriety be stated to be essentially the modern theory of the mechanism of parturition."

Leaving, then, for the present, our author's views regarding the frequency of the various positions of the head at the brim, let us see what he has to say concerning the mechanism of labour—premising, that he confines himself almost entirely to the consideration of that of vertex positions; and that he holds there are, essentially, but four such positions, all of them oblique: that is to say, with the opposing diameter of the presentation engaging an oblique diameter of the pelvic brim. Two of these positions are occipito-anterior and two occipito-posterior.

The author almost exclusively confines himself to the consideration of the mechanism of the first position; the left occipito-cotyloid, or if it be preferred, the left occipito-thyroid. A position in which

the occiput corresponds to a place behind the left cotyloid cavity, or left thyroid foramen, and the synciput is opposite the right sacro-iliac synchondrosis.

Prefatory to his observations on the mechanism of this position our author defines certain terms, which he considers are used without scientific accuracy, such as "vertex," "presentation," and "at the brim."

Dr. Leishman would wish us to understand "the sagittal suture in its whole length, and the parietal *bone* (once called *os verticis*) as far as *its* tuber, as the vertex in midwifery; as by so doing" he "shall be able to term the whole four positions, in all stages and varieties, as true presentations of the vertex." Had our author said, "sagittal suture in its whole length, and *both parietal bones* as far as they can be felt," we conceive he would have been nearer the mark. We confess, however, that in our mind we have never had any difficulty in comprehending the term vertex, that is, in its obstetric sense. We consider "vertex," in midwifery, to mean the whole circumference of the arch of the fetal skull; but that, the quantity of the presenting surface of this arch varies according to the period of the second stage of labour at which we make an examination. Thus, as the head enters the brim, "the vertex" is an ovoid whose long diameter is formed by the occipito-bregmatic diameter of the fetal skull, and whose short diameter is its bi-parietal. When the face has turned towards the hollow of the sacrum, and the occipital end of the oval becomes fixed at the pubic arch, "the vertex" gradually becomes more and more extensive as to its surface (because the face then sweeps the side of the hollow of the sacrum, and the arch of the skull distends the perineum), till at last the vertex may be defined as the complete arch of the skull—the long diameter of the oval now being the *occipito-frontal*, and the short the bi-parietal. Even though from obliquity a portion of the fetal head below one or other of the extremities of the bi-parietal diameter, may occupy the pelvic space, we have never experienced any difficulty in appreciating the mechanism of the four positions by thus defining the term "vertex." We think it a mistake to be over-refined as regards this term.

Now, with respect to the word "presentation," a term, it appears, which has caused our author much trouble, he says:—"That the difficulties are even greater, and the confusion amongst various writers more embarrassing," with regard to this term than with respect to the term vertex. "If," he continues, "we imagine a

series of planes radiating from a common centre in front of the pubes, and passing to the posterior part of the pelvic wall—the upper of these being the floor of the brim, the lowest that of the outlet—the parabolic curve which passes through the centre of these is manifestly the axis of the pelvis. Now, if we are forced to name a *point* as the *presentation*, we might, with some show of propriety, adopt that definition which limits the presentation to that part on the surface of the child's head through which the axis of the pelvis passes. But the objection to this is to be found in the fact that it is practically impossible to determine this with anything approaching accuracy; and, therefore, we must adopt, for our information and guidance in practice, some more simple, if less accurate, idea of the term." We confess this appears to be much ado about nothing. Why confine the definition to a point through which a certain axis passes, even if one could do so with scientific accuracy? That point must be, after all, only a point *on the presentation*. Moreover, we cannot agree with Dr. Tyler Smith's definition of this term, which our author seems forced to adopt, viz.:—"That it would be best to define the presenting part, in every kind of cranial position or presentation, as *that portion of the fetal head felt most prominently* within the circle of the os uteri, the vagina, and the ostium vagina, in the successive stages of labour." We object to this definition because it still confines itself to a particular spot on the presentation, viz., so much of it as is "*felt most prominently*." Why make a difficulty where no difficulty exists? The presentation, say in the case we are about to consider, is "the head" the *order*, "vertex." No very minute definition is required of this word, whether relative to the head or any other part of the fetus. We would understand presentation to mean, whatever portion of the fetus engages the os uteri, the brim, the cavity, or the outlet of the pelvis, and as much as can possibly be felt, or is capable of being felt, at any of these situations.

The next, and last, term which our author conceives to be vaguely defined is, "at the brim." He considers "that the head should be described as 'at the brim' until its bulk has passed this part, which does not take place until the head has begun to experience some resistance from the floor and converging walls of the pelvic cavity." We consider, instead of rendering matters more simple, this definition of the term increases our complexity. Should we take this as a true definition, we might give ourselves

credit for delivering a head with the forceps while "at the brim," when in reality the most of it would be in the cavity.

In our mind there can be no difficulty about all the terms in which the word brim is used. For example, the term "head above brim," we consider to mean that the head has, in no part, as yet entered it. Such a condition is peculiarly appreciable in cases of narrow brim. Head "at the brim" means, that the occipito-bregmatic oval has just entered it; and this term is identical with "just entering the brim." "In the brim," when the opposing diameter has unmistakably engaged that circle; and this is a situation not so frequently found as the following, viz., "passing the brim." We consider the head to be passing the brim (not "at the brim") until the cavity of the pelvis has been occupied and the perineum is just commencing to be pressed upon, at which period the head may be said to have "passed" or "cleared the brim."

And now to return to the author's ideas relative to the first position of the head at the brim, his views being grouped under the three obliquities above described in connexion with that position.

First, then, with respect to the obliquities of Saxthorpe and Solayrés, whereby a long diameter of the head occupies an oblique diameter of the brim—in other words, the head is turned on its vertical axis. Here it is essential to quote our author at length, in order to do him justice:—

"That the head, in the vast majority of cases, presents itself at the beginning of labour in a direction approaching to the oblique of the brim, and that as labour advances it passes more decidedly into that diameter, are facts almost universally admitted. There are, however, some obstetricians of eminence, among whom I may mention Cazeaux and Ramsbotham, who consider that the transverse position occurs so frequently as to warrant our retaining it in a system of classification. I am perfectly willing to admit that, with a wide pelvis and a small head, there is scarcely any position that may not occur; but I think the weight of the evidence is entirely against the probability of the transverse position being anything but extremely rare in its occurrence. It is well known to all accoucheurs that frequently (but by no means *always*, as Nægèlé says) the ear may be felt at the beginning of labour immediately behind the pubes; and it is important that the exact diagnostic value of the ear in this situation be exactly determined. On this subject Dr. West, in his essay, has various important remarks, which I have, to some extent at least, had an opportunity of confirming. It is quite evident that this frequent position of the ear behind the symphysis pubis is often

looked upon as evidence that the other ear is in the middle line of the sacrum, and, consequently, that the long diameter of the head is in the transverse of the brim; but, so far from that being the case, he says that on finding, for example, the right ear in this position, and indicating that the face is to the right, we may be sure that the head is oblique and in the first cranial position. I cannot, I confess, go quite so far as Dr. West; but I have, on more than one occasion, introduced my hand when the right ear was in this position, and found the left—not, indeed, as he says, ‘at the left ilium, or thereabouts,’ but certainly, and very decidedly, to the left of the promontory of the sacrum. This subject requires and deserves further investigation; but I am inclined to believe that the position is in most cases indicative only of that situation intermediate between the transverse and oblique diameters of the brim, which the head so frequently occupies in the early stages of labour.

“It is undoubted that early in labour the position is frequently more transverse than it is at a later period, but it is by no means easy to ascertain what is the cause of this alteration. I am inclined to think that it is determined, to some extent at least, by the shoulders of the child coming in contact with the projecting lumbar vertebræ, aided, it may be, in the case of a living child, by the motions of the superior extremities. It is conceivable, at least, that the motion of the arm of the child should tend to turn the shoulder, and consequently the head, into an oblique position, seeing that the arm next the lumbar vertebræ would find a resistance to which nothing analogous exists on the smooth wall of the uterus. It is certainly difficult to understand how, without the operation of some such cause, the long diameter should turn at so early a stage of labour from the transverse position into one where, taking the soft structures into consideration, it actually has less room.”

With respect to transverse positions of the head at the brim, or as it is entering it, we perfectly agree with our author, viz., that such a position may occur when the pelvis is large, out of proportion with the normal fetal head, or when the head is preternaturally small while the pelvis is normal; under either of which circumstances we may have a *variety* of vertex positions, all of which may be said to go for nothing so far as the mechanism of labour is concerned. But when the fetal head and mother's pelvis are in due proportion one towards the other, at full term, it is highly probable—indeed we have observed it repeatedly—that the head always occupies a more or less transverse position *prior* to its entering the brim—i.e., the occipito-bregmatic diameter of the head nearly, *but not exactly*, corresponds to the transverse of the brim. That the opposing diameter of the head, however, occupies an oblique diameter of the

brim when it *is* entering, or *has* entered, this space, is undeniable. Therefore we ignore transverse positions in classification. The fact of the fetal head corresponding nearly to a transverse diameter of the inlet before it becomes engaged therein, may, in some measure, account for an error which has lately been enunciated in our school, viz., that the face of the child in early labour is *always* anterior.

As to the reason *why* the fetal head takes an oblique position, much has been conjectured by various writers; we cannot, however, agree with the suppositions of our author. We do not consider that the shoulders contribute in any way to the arrangement of the head in the oblique position; and we deem it less probable that the motions of the child's arms tend to produce a similar condition. It seems to us that this obliquity may be more easily and satisfactorily accounted for. It must be remembered that in accounting for the mode in which the fetal head enters the pelvic brim, we should take into consideration what effect may be produced by the play of the smooth rounded surface of a movable ovoid body against a similar surface of a fixed ovoid body. The movable ovoid is formed by the fetal head, and the fixed one by the sacro-vertebral angle. This latter encroaches upon the conjugate diameter of the brim posteriorly, overhanging that obliquely-placed plane; but there is at each end of this rounded projection considerable space, in a direction backwards and outwards, corresponding to the region of each sacro-iliac synchondrosis.

If, then, we suppose, as is the case, that a power, acting from above downwards and obliquely backwards, pushes the movable oval into the brim thus coercted antero-posteriorly, the movable oval will oscillate upon the fixed one, and the end of the former oval which is nearest to one of the ends of the latter will turn into the space there situated, and so apply its long diameter to that diameter of the brim where it is best accommodated, viz., the oblique. Thus in the early stage of the first position, the occiput is somewhat in the anterior half of the pelvic brim, and to the left, while the synciput is somewhat in the posterior half, and to the right; therefore the synciputal end of the head's oval, the movable one, is the end nearest to the right extremity of the oval formed by the sacro-vertebral angle, the fixed one. When, therefore, force is applied by uterine action, the movable oval of the head oscillates upon the fixed one formed by the sacro-vertebral angle, it fills up the space there available, and so comes to occupy the oblique diameter of the pelvis or left occipito-cotyloid.

The oblique diameter of the pelvic brim in the living subject we consider to be the longest, and that which will best accommodate the opposing diameter of the head; so that here we are quite opposed to the author when he says that the head when in the oblique diameter, "taking the soft structures into consideration," has "actually less room than in the transverse."

It is quite true that the transverse diameter of the brim in the skeleton is longer than the oblique, but in the living subject the reverse obtains, and from the very fact of the presence of these soft parts. The muscular structures which occupy the iliac fossæ encroach upon the transverse diameter, and change the shape of the brim into that of a triangle, the base being posteriorly and the truncated apex somewhat anteriorly; therefore the oblique diameter of the brim in the living subject becomes longer than the transverse.

The next point of Nægèlé's touched upon by the author is the biparietal obliquity of the first position, by which the head is turned "upon its occipito-frontal axis" so as to bring the ear towards the left shoulder. Our author tells us that, though he began his "study of the subject with the firm conviction that Nægèlé was right in this particular," he has been, "step by step, driven to the conclusion that he is perfectly wrong." However, Dr. Leishman does not wish us to suppose that in his views on this department of the mechanism he is original. He thus, in our opinion, most correctly argues:—

"In admitting the general accuracy of most of Nægèlé's descriptions, I assume that the fundamental error from which, more than any other, his mistake arose, was an ignorance, at the time he wrote his essay on the subject, of the great obliquity of the brim in respect to the horizon. There must, I think, have been remaining in his mind some remnant of the old idea of the *horizontal* brim; for it must be remembered that his attention was not directed to the subject of the relation which the pelvis bears to the trunk and limbs until some years after the date of the publication of his paper on the mechanism of parturition. If the brim were, indeed, parallel to the horizon, or nearly so, the fact of the finger meeting the parietal bone in the vicinity of its tuber would be clear and irrefragable evidence of the so-called lateral or biparietal obliquity of the head. But if we do not allow ourselves to lose sight of the fact that the brim is inclined at an angle of 60° , and that the vertex, or presenting part, passes downwards and backwards so obliquely as to meet the horizon at an angle of 30° .—even admitting that the right parietal bone

in the vicinity of its tuber is the lowest part in the pelvis—I cannot see how this is to be accepted as evidence of anything else than that the head is advancing directly in the axis of the brim, but very obliquely with regard to the cavity, and still more so with reference to the horizon.”

Our author here quotes Nægèlé's opinions, and then proceeds, in a masterly manner, to show up their weak points; and, although he has not afforded us any new ideas as to facts in this portion of his work, yet argues each topic in a way as original as it is clever. With his views we thoroughly agree.

The next subject touched upon is the occipito-frontal obliquity, viz., whereby the occipito-frontal (more properly speaking, the occipito-bregmatic) diameter of the head is made to correspond with the oblique of the brim—in one word, flexion of the head. And here we may observe that our author never seems to acknowledge the existence of the occipito-bregmatic diameter of the head as the opposing one, *i.e.*, the diameter from the occipital protuberance to the centre of the anterior fontanelle. This is the true opposing diameter in the position he is examining, and not the occipito-frontal, which latter is measured from the same point on the occipital bone to the centre of the top of the fetal forehead; a longer diameter than the occipito-bregmatic. The flexion of the head and the depression of the chin on the top of the thorax, is the cause of this being the opposing diameter of the head, and it is the shortest of the head's long diameters. But, as regards the origin of this flexion, our author observes it has never been better explained than by Solayrés. “To understand this,” he continues, “it is only necessary to remember that the force is communicated to the head from the uterus and other expulsive muscles through the vertebral column of the child; and again, that it is articulated much nearer the posterior than the anterior part of the base of the cranium. Consequently, if the resistance which the pelvis offers to the advance of the cranium be equal at both ends of the ovoid, it is certain that, unless other forces come into play, the posterior end of the occiput must precede the other; the chin becomes firmly pressed against the chest, and the mobility of the head being thus checked, the expulsive force is applied at a greater mechanical advantage.” All this is, no doubt, very ingenious; yet we are under the impression that we could demonstrate how the head occupies this flexed position towards the latter end of gestation, and long before labour has set in.

Our author next proceeds to observe upon the passage of the fetal head through the cavity of the pelvis; and with respect to this portion of his treatise on the mechanism of labour, we are presented with nothing new. Thence we are led to the behaviour of the head as it passes the outlet. Some, it appears, and amongst them Dr. West, consider that the head is born with its long diameter—antero-posterior—exactly in the conjugate diameter of the outlet; whilst Dr. Matthews Duncan asserts, that the “lateral obliquity of the fetal head, when passing through the outlet of the pelvis,” was “not described by Nægèlé and his followers,” but that it “does occur.” Now, Dr. Leishman *proves* that Nægèlé *did* teach this obliquity of the head as it passes the outlet, and that Nægèlé was right in so teaching, the head always passing the outlet in an oblique direction. From the following passages will be gleaned the author’s views relative to this part of the process, and also the method by which he arrived at them:—

“In point of fact we may, with perfect propriety, describe here, as Nægèlé and his followers do at the brim, three kinds of obliquity. The first is that by means of which the occiput is forced in advance of the forehead, the latter being arrested at the inferior part of the sacrum; the second causes the long diameter of the head to occupy a position intermediate between the oblique and conjugate measures of the pelvic cavity and outlet; and the third is that which we have just described, which causes the right parietal protuberance, in the position which we are considering [*viz.*, the first], to precede the left in passing the place of the outlet. It is a much easier matter to investigate the position of the head at the outlet than to determine its relations at the brim, as we can in the former case refer the position to real determinate diameters, and not to mere imaginary lines, in which we have, at best, nothing approaching to mathematical accuracy. The manner in which I have studied the positions of the head at the brim is sufficiently simple, and at the same time almost free from the possibility of error. If one end of a cord be held by an assistant in close relation with the tip of the coccyx, and the other be carried along the middle line of the vulva to the centre of the symphysis pubis, this line must indicate, with sufficient accuracy, the conjugate diameter of the outlet, or, in fact, that diameter whose relation to the head chiefly enables us to determine the position of the latter.

“I must premise, with reference to the position of the head which I have observed in this manner, and which I am about to describe, that it must be considered merely as the ordinary position of the head in a labour of the first kind, the relative proportions of the maternal parts and

the head being normal. Putting aside all deformities at the outlet, the circumstances in which—other things being equal—we find these various obliquities most strongly marked are presented in the case of a woman who is above thirty-five years of age, and in labour for the first time; and here, also, by reason of the slowness with which the concluding stage of labour is effected, we can best examine the whole phenomena, including the caput succedaneum. On the contrary, in multiparæ the ligamentous and soft parts are often so relaxed that the head passes through almost—and, I believe, in some cases altogether—directly, without availing itself of any of the obliquities except the first described. What may be called the normal process is intermediate between the two. When the caput succedaneum, or presenting portion of the head, first appears between the vulva, and comes to press upon the cord steadily held in the position above described, a line may be drawn, by means of a camel's hair pencil and a little ink, using the cord as a guide, and carrying it as far over the presenting part as may be possible during the height of a pain. This line being produced in both directions after birth indicates that portion of the head which answers to the conjugate diameter at the time when the head first makes its appearance between the vulva, and is represented by a line drawn from about the middle of the right limb of the lambdoid suture, over the vault of the cranium, to the most projecting part of the left division of the frontal bone, or a point slightly external to this. This line crosses the sagittal and coronal sutures, the former at a point much nearer the anterior than the posterior fontanelle. At this period the right parietal protuberance may be felt behind the tuberosity of the right ischium, while the corresponding point on the left parietal bone may be felt considerably higher, near the inferior margin of the sacro-sciatic ligament, and on the level with the tip of the coccyx."

We have quoted this passage at length, as we could not in our words lay before our readers the author's opinions so clearly as he does in his own.

Dr. Leishman now flies off at a tangent to the consideration of a most important subject, namely, the treatment of labour towards the termination of its second stage; the great question being—"Should the perineum be supported?" but as we have already occupied considerable space we shall postpone our review on this and the concluding chapter of his work, about which we have a great deal to say, till the next Journal.

Practical Dietary for Families, Schools, and the Labouring Classes.

By EDWARD SMITH, M.D., LL.B., F.R.S., &c. London: Walton & Maberly. 1864.

THE above work, says Dr. Smith, "is intended to be a guide to heads of families and schools in their efforts to properly nourish themselves and those committed to their care; and also to clergymen and other philanthropists who take an interest in the welfare of our labouring population. It is essentially practical and popular in its aim, and, therefore, contains directions rather than arguments; but at the same time it is based upon the most advanced state of the science, and, except in the exclusion of many technical terms, is scientific as well as popular."

In accordance with this programme few authorities are cited, the chief being chemical ones, such as Messrs. Lawes and Gilbert Professor Playfair, and M. Payen.

Part I. treats of "Foods," and is divided into three chapters. Chapter I. treats of "the elements of food which the body requires." Here are popular, but correct, dissertations on the entrance of food and its uses within the body; on the emission of waste from the body; on the composition of the structures of the body; and on the quantity of the elements of the body required daily. The importance of diffusing correct notions among the public on these matters can scarcely be overrated; and it is due to Dr. Smith to say that he has done this part of his work well and wisely. We can understand the difficulty he must have felt in popularizing so scientific a subject as that of Chapter I.

Chapter II. gives a summary statement of the elements of the body which can be supplied by food; while Chapter III. treats of the qualities of foods. Under each head in this chapter we find brief statements respecting the origin, nutritive qualities, preparation, and cooking of the various kinds of food. Thus, under Section I.—Dry Farinaceous Foods—a great deal of useful information is given about wheaten flour, the relative value of wheat grown in various climates and latitudes, the usages of millers in mixing the various kinds, the value of the bran, the market prices of various kinds of flour, and the reasons for those prices; also, the nutritive values of various kinds of wheat, and the mode of preparing flour as food. The best mode of baking, the economy of baking at home or buying bread, and the age at which bread should be eaten, are all discussed with simplicity and clearness. For example, at p. 43 we read:—

"It is often asked whether it is better to bake at home or to purchase baker's bread. Assuming that it is convenient to bake at home, it cannot be doubted, I think, that it is better to do so. If the flour be good the bread will be unadulterated; it will keep longer than baker's bread, and the flavour [will] be generally preferred. On the other hand, it must be stated that sad or heavy bread is sometimes produced at home; the baking is less uniform than at a bakehouse, and if any portion be under-baked it is indigestible; whilst baker's bread can be obtained in many districts of uniform quality throughout the year. When, however, home-made bread can be always well made, and the flavour is approved, it will generally be more healthful to provide it."

Respecting the age at which bread should be eaten, Dr. Smith objects to the use of new bread, because it is less digestible than the stale article. This indigestibility of new bread, he thinks, is owing "to its greater tenacity leading to less perfect mastication;" and "is not so much due to any chemical quality" as "to its physical constitution." He then proceeds as follows:—"After this period [the first day] the changes proceed more rapidly in baker's than in home-made bread, so that the former will often taste sour on the third day, and will then have lost much of its agreeable flavour; whilst home-made bread will remain sweet and agreeable in flavour for a week, and during that long period [will] have exhibited no change other than increased dryness and solidity. I think the plan of thrifty housewives in this matter is based upon correct observation, viz., that baker's bread should not be eaten until the second day, and rarely after that period, whilst home-made bread may be eaten on the second and third day, and extended to the fifth or sixth day." Oatmeal, rye and barley, maize, rice, peas and beans, sago and arrowroot are also treated of in this section.

Section II. of Chapter III. treats "of fresh vegetables." At the head of this class stands the potato, which is referred to with due respect. We are told of the various kinds of potatoes, the best kind of soil for growing them, the best mode of cooking and peeling them, and the weight of them which alone would supply the daily nutriment required by a man. This is stated to amount to "about 6 lbs. in reference to the carbon, and 8 lbs. in reference to the nitrogen."

Green succulent vegetables are considered under one head. Parsnips are made to rank next to potatoes in nutriment, and are followed by carrots, Swedish turnips, onions, and common turnips.

Section III. SUGARS.—The various varieties of sugar, their

modes of refinement, its fattening properties, and the most economical mode of using it, are here fully discussed. "A medium price of $4\frac{1}{2}$ d. to 5d. per lb. is the most economical quality." Treacle is also mentioned, and its nutritive value given. It "has the same curing properties as sugar, and is largely used in America for that purpose."

Section IV. FATS.—Olive oil, butter of various qualities, salt and fresh, dripping, lard and suet are duly noticed; and it appears "that whilst butter is the most universally prepared separated fat which is accessible to the inhabitants of this country, it is by far the dearest; and that whilst dripping is the least obtainable it is by far the cheapest."

Section V. MEATS.—Dr. Smith here takes it for granted that a certain portion of meat is a very useful element in a dietary, and does not discuss the views of vegetarians, respecting which he refers to his work, *Health and Disease as influenced by the Cyclical Changes in the Human System*. Lond. 1861. Butcher's meat occupies no small share of attention; and as regards cooking it, he affirms that the object is not to change the composition or nutritive value of the meat, but to present it to the palate in a form which is agreeable, and to the stomach in the form most fitted for digestion. "Hence as small a portion as possible of it should be destroyed by the heat, or removed by the water, and no part should be made much harder than it was before the heat was applied." Boiled or stewed meat, when properly cooked, in all probability is more digestible than roasted meat; and in some of Dr. Smith's experiments it was found "that thirty per cent. of all the solid matter left in meat liquor was salts of the greatest value in nutrition." The relative monetary value, and the due exercise of economy in the purchase of joints are minutely and most usefully entered into; and respecting the South American beef recently imported into these countries, and the preserving process patented by Mr. Morgan of this city, Dr. Smith remarks:—

"I think it very doubtful if the South American beef, as at present imported, will be accepted as food even by our poorest classes. The specimens which have been sent to me have been exceedingly dry, and by no process of cooking could they be rendered agreeable food. It is true that in a given weight they contained far more nitrogen than is found in fresh meat, but that was owing to the juices having been evaporated by the drying process; and as the fibre is hard, and can be masticated only with difficulty, it is impossible to admit that nitrogen in that form is at

all to be compared with the same element in fresh meat. Hence the statements as to the nitrogen which it contains are no evidence of the nutritive value of the meat. If it can be brought to this country in a comparatively moist state, without its having been much salted, and with a fair proportion of fat to the lean, it will doubtless be a great boon to the masses of the people. How far the preserving process, so ingeniously applied by Mr. Morgan, will answer this purpose yet remains to be fully proved; but if it should succeed in preserving the meat, the quantity of salt which is introduced, and the comparatively short time in which the meat must remain ere used, will not deteriorate the food to so great an extent as now occurs in an ordinary process of salting. So far as it has yet been tried I think it a very valuable process."

We next find mention of bacon, tripe, cow-heel, sausages, and black puddings. Concerning black puddings, which are prepared from the blood of animals, Dr. Smith states that the best kind is prepared from *pigs' blood, groats, and pork fat*; and he refers to the strong repugnance to the use of blood which still exists among many, chiefly in consequence of the Scriptural prohibition to the Jews. This prohibition, we may ourselves observe, is by many Christians believed to extend to us all;^a and Dr. Smith considers that the immaturity of blood renders it an undesirable food.

Section VI. treats of fish; Section VII. of gelatine; Section VIII. of eggs; and Section IX. of cow's milk. Under this last head we find minute information about new milk, skim-milk, butter-milk, and whey; and Section X. treats of Cheese.

Section XI. Tea, Coffee, and Chicory.—Here Dr. Smith recommends that tea should be prepared by a weak infusion, and drunk in small quantities, after the Chinese fashion. He states that tea is *useful* to the corpulent; the over-fed, after a full meal; at the end of the day, when food has accumulated in the system, when digestion and other changes proceed slowly; for the old; for hot climates; for the sedentary; for those who do not perspire freely; for those who eat much starchy food; for soldiers on the

^a The Mosaic law, regarding the use of swine's flesh, things strangled, and blood, appears to be founded on sound sanitary principles; when we recollect the recent discoveries regarding entozoa in pigs, the tendency of the consumption of the flesh of these animals to predispose to cutaneous disease, and the injurious properties of blood in the diseases of that fluid.

The decree of the Synod of Jerusalem, recorded in the Acts of the Apostles (xv. 23–29), abolished the obligations of the Mosaic law on Gentile converts, except in the prohibitions regarding things strangled, blood, and fornication, which are expressly continued. According to some of the best critics the word *πορνεία*, translated "fornication," (Acts xv., 29) should be more correctly rendered *pork*, or *swine's flesh*.

march in hot climates; and as a restorative in cases of drowning; or wherever it is desired to increase the respiratory functions. Further, he observes that tea is *hurtful* in the absence of food, after a long fast, to the poor and ill-fed, the spare, and the young. It is not adapted to sustain exertion, to prison dietaries, to low temperatures, or to hot climates, when the appetite is defective, and the skin active, or to those who perspire too freely; neither should it be taken with our principal meal. Coffee Dr. Smith regards as more fitted for breakfast, while he thinks tea more fitted for the requirements of the body at night.

Section XII. ALCOHOLS.—Respecting ardent spirits, Dr. Smith says:—"The proper place for these compounds is as medicines, and as such their value may be as great as their power; also as luxuries, but not as foods, and they should not find any place in mere dietetic arrangements." On WINES he remarks:—"I do not think that wine can be regarded as a necessary food, but on the other hand I am convinced that its use is quite unnecessary in the ordinary conditions of health." Beer, and porter, and cider, and perry are noticed; and Section XIII., on CONDIMENTS, concludes Part I. In this last section it is stated that, "the required quantity of common salt is from $\frac{3}{4}$ to $\frac{1}{2}$ oz. daily."

Part II. DIETARY, consists of three chapters, and an appendix. It commences with Chapter IV. THE DIETARY OF FAMILIES. The statements here made are addressed exclusively to those who are able to obtain, and do obtain, a suitable quantity and variety of food for their families, and to whom extreme economy in the selection of food is not of any importance. The remarks contained in Section I. on Dietary in Infancy, are specially valuable. Clear instructions are given to mothers and nurses, and common mistakes pointed out. The same observations apply to Section II., on Dietary in Childhood, in which the quantity, quality, variety, mode, and time of administration of food are discussed at length, as well as in Section III., in which the Dietary in Youth is dwelt on, with particular reference to the hours of meals, and the use of tea and alcohols. Section IV., Dietary in Adult and Middle Life, not only contains practical teaching, but also bills of fare suitable for adults and middle-aged persons. Notes are given for those *who dine at a late hour*; for those *who cannot eat much breakfast*; for those *who are of feeble constitution*; for those *who lead a sedentary life*; for those *who suffer much anxiety*; for those *who make much exertion*; for *wet nurses and nursing mothers*; for those *who labour in the*

night and rest during the day, e.g., readers, compositors, and others employed in printing offices; for those who wish to *increase*, and for those who wish to *decrease in weight*. A good many pages are devoted to the cases of the spare and fat persons just noted. Those who wish to *increase* in weight are advised to use new milk, sugar, fat, highly-fed meat, "the addition of two or three teaspoonfuls of brandy or rum to the milk or the chocolate several times a day, and the use of good and new home-brewed ale." Those who wish to *decrease in weight* (attend, all ye disciples of Mr. Banting) are advised to increase waste and to decrease supply. To secure the latter desideratum very little or none of the following articles should be used, viz.:—Milk, fluids generally, butter, lard, dripping, suet, oil, sugar, flour, and almost everything that people like to eat and drink. A fixed form of dietary is given, but it is too long to transcribe to our pages; and the "training" necessary to accomplish the same object is delineated, with special reference to King, Heenan, and that brutal pastime which the savage and degraded spirit of this nineteenth century calls "the noble art of self defence."

Section V., Dietary in Old Age, concludes Chapter IV.

Chapter V. Dietary in Schools.—We shall not spoil this most useful chapter by quoting any of it. It ought to be all transcribed, did space admit; or, better still, it ought to be re-printed as a pamphlet, with the notes on the dietary for young persons, in Chapter III., and circulated by the thousand. Would that schools were properly *inspected* as regards dietary. Had this been so, Mr. Dickens would never have written *Nicholas Nickleby*, to show up to an indignant public Mr. Wackford Squeers, of Do-the-boys Hall, and his amiable partner, who, in administering the treacle and sulphur to the boys, to take away their appetites, argued, that she served them and the economical interests of the academy at the same time

Chapter VI. treats of the dietary and family arrangements of the labouring classes, and contrasts, in Section I., the present mode of living of a family—infant, young children, wife, husband, single young men, and women, and youths—with "the most suitable dietary," as given in Section II. Section III. gives "the best arrangement of meals," and Section IV., at much length, "Specimen and proposed Diets." Chapter VII. gives copious directions for the management of Cooking Depôts in Section I., and of Soup-kitchens in Section II. The Appendix for the poor, is, perhaps the most valuable part of this book. It consists of copies of

proposed handbills for popular distribution by parochial clergymen and medical men. A very copious Index concludes this volume of 265 pp., small 8vo.

In connexion with the subject of food for the labouring classes it may be well to remind our readers of a series of valuable papers in former numbers of this Journal, in which the Rev. Professor Haughton, in conformity with the experience of the mass of mankind employed in manual labour, in all ages and countries, urged that men employed only in manual or routine bodily labour are sufficiently well fed on vegetable diet, while the *opus mentale*, as opposed to the *opus mechanicum*, requires a better quality of food.^a

We most sincerely recommend this book on Practical Dietary to the perusal and *digestion* of every class in the community. The Prime Minister will get most useful information from it; the clergyman who uses it will become *popular* and *useful* at the same time; the physician will find good prescriptions in it for the treatment of wearying individuals mis-called *patients*; and the peasant, if he can but read it and act on its directions, will become a happier and a healthier man.

Ligature of the Left Common Iliac Artery; being the Second Operation in Ireland, and the First Successful Case of it.—By WILLIAM HARGRAVE, A.M., M.B.; Professor of Surgery, R.C.S.I., &c. Pamphlet.

THE facts of this very remarkable case may be very briefly epitomized as follows:—

The patient, a man, aged forty-three, had the usual symptoms of aneurism in the left iliac fossa. The tumour had been observed for about six months, and finally attained a diameter of five inches. It was somewhat triangular in form; the apex running into the common iliac artery, and one side dipping down into the pelvis. Compression by Carte's apparatus, by L'Estrange's, for compressing the abdominal aorta, and by the finger, were each tried, and found ineffectual, even with the aid of chloroform, opium, digitalis, restricted diet, &c. Compression on both distal and proximal portions of the main arteries connected with the tumour was also tried, with the same want of result. Direct pressure on the tumour and flexion of the thigh at the hip-joint also failed; so also did the

^a On the Phenomena of Diabetes Mellitus. Aug., 1859 and 1860.

sedulous use of ice; nothing therefore remained but the ligature of the common iliac artery. This artery seems to have been tied in thirty-two cases previously to Mr. Hargrave's; of these but six recovered. Whether Mr. Hargrave's case is to be considered successful or not our readers must decide for themselves.

The vessel was ligatured on the 29th April, 1865. The line of incision was eight inches in length, and reached from the point of the last rib to the centre of a line drawn from the spine of the ilium to the symphysis pubis. The peritoneum gave no trouble, separating more readily than in the dead subject, and bringing with it the ureter. There was nothing further of note in the operation itself, if we except the great sensitiveness of the genito-crural nerve, which lay upon the common iliac artery, and required some little manipulation to separate it. The embarrassment sometimes caused by the intestines bulging over the line of wound was prevented by gently rolling the patient towards the opposite side.

On the ninth day some pulsation returned in the aneurism, but without bruit or thrill; on the twenty-ninth day the ligature came away; the discharge from the wound almost ceased. On the thirty-first gangrene of the outer part of the foot set in; this slowly increased, and was at the last accompanied by pelvic abscess of large size; change of air and everything that skill could suggest in the way of food or medicine, was tried, yet the man finally sank on the seventy-third day, mainly owing to hemorrhage from the portion of the abscess in the right pelvis. The wound remained flabby and disinclined to heal throughout.

Post mortem examination confirmed the opinion previously formed, that this was a case of aneurismal varix, the aneurism communicating with the external iliac vein by a well-defined oval opening of a quarter of an inch in diameter.

Mr. Hargrave claims this as a successful case, so far as ligature of the vessel, and believes that the fatal issue is due to pyemic causes, resulting from the accidental gangrene of the foot. The *post mortem* showed that as far as the sac was concerned the process of cure was far advanced; and even if the compilers of statistics should hesitate to class it among the recoveries, it is to be hoped they will not overlook the satisfactory parts of the operation, so far as the safe application of the ligature and its subsequent coming away without hemorrhage are concerned. It gives encouragement to other surgeons to follow Mr. Hargrave's steps in the performance of this brilliant and daring operation.

- 1 *Orthopraxy: the Mechanical Treatment of the Deformities, Debilities, and Deficiencies of the Human Frame. A Manual.* By HEATHER BIGG, Assoc. Inst. C.E.; Anatomical Mechanist to the Queen and Prince of Wales, &c., &c. London: Churchill, 1865. Crown, 8vo, pp. 709.
- 2 *A Practical Treatise on Rupture: its Causes, Management, and Cure, and the Various Mechanical Contrivances Employed for its Relief.* By T. P. SALT, Birmingham; Anatomical, Surgical, and Orthopedic Mechanician; Surgical Instrument Manufacturer to the General and Queen's Hospitals, &c. London: Churchill, 1865. Crown 8vo, pp. 112.

WE associate these works in one notice because they are both written by surgical-instrument makers; and we believe the publishing of such books by instrument makers is not only a mistake on their part, but calculated to be injurious to suffering humanity. We do not say one word as to the impropriety of instrument-makers, who have received instructions from surgeons as to the fabrication of surgical apparatus, publishing descriptions of these with remarks on the cases in which they may be employed, with a view to attracting customers or "patients" to their shops; but on the part of humanity and of medical science we protest with all our might against cutlers and instrument-makers, whether they be Associates of the Institution of Civil Engineers or not, undertaking the treatment of disease, and by screws, and springs, and ratchet-wheels forcing and contorting the body into various shapes, independently of all considerations of vital or constitutional peculiarities.

In another article in this present number of the Journal we have dwelt on the anatomy, pathology, and treatment of spinal deformities, and shown in how great a degree these depend on constitutional causes, and require in their treatment all the skill and judgment to be derived from the patient study of disease. Mechanical apparatus is no doubt an important part of the treatment, but still it is a secondary one; and certainly the greatest injury that could be done to the manufacturers of such apparatus would be to attempt to elevate them into medical practitioners.

While thus objecting to these works, we do not deny that much valuable information may be obtained from them; but it is somewhat remarkable that in two instances at least—hernia and prolapsus of the uterus—neither of the authors makes mention of the instruments most commonly used and most valued by Dublin surgeons.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.^a

DR. M'DOWEL, President.

Disease of the Supra-Renal Capsules.—DR. HAYDEN said he had the honour of submitting to the society an example of Addison's disease, which presented some peculiar, and as it seemed to him, exceptional features. The patient was a woman fifty-eight years of age, and was the mother of a large family. She was admitted into the Mater Misericordiæ Hospital on the 9th of last October. The history which was obtained from her was the following:—She had enjoyed comparatively good health from infancy up to two years before her admission, with the exception of occasional cough and expectoration. About two years previously she suffered from a cough of a more decided and severe character, with expectoration. She complained of severe pain in the right scapular region, which lasted three weeks, and was so severe that she could scarcely breathe whilst it continued. Three months subsequently to this time, that is twenty-one months previously to the date of Dr. Hayden first seeing her, she observed a slight discolouration of the chest and arms, and subsequently of her face. A month previously she suffered for the first time from epigastric pain, accompanied with retching and vomiting, irrespectively of taking food. One week before he saw her she was attacked with diarrhea. She occasionally expectorated a little blood; and about three weeks before the date of her admission threw up a large quantity of florid blood. When he saw her first she was exceedingly weak. She could hardly stand erect, so great was her debility, without support; and when she attempted to do so, had a swimming in the head, and was about to fall. The pulse was 96, regular, but exceedingly feeble. The woman complained of pain in the epigastrium, with constant retching, and was unable to retain any food,

^a These reports are furnished by Dr. R. W. Smith, Secretary to the Society.

solid or fluid, in the stomach. Her tongue was, nevertheless, moist, but slightly furred. The face presented an almost uniform nut-brown colour; less so, however, upon the cheeks and upon the forehead. The sclerotics were pearly white, and the upper part of the chin was likewise white, as were also the margins of both lips. The neck, chest, and upper part of the abdomen were covered with large brown blotches, with intervening spaces of uncoloured skin, which were perfectly white. The lower part of the abdomen, within a space bounded by a line extending from an inch and a-half above the umbilicus on each side to the crest of the ilium, and below by the crest of the pubes and Poupart's ligaments, was perfectly white, as were also the entire of the lower extremities, with the exception of the upper and inner part of the thighs, and adjacent portion of the perineum, which were of a deep brown colour.

The anterior aspect of the upper arms was likewise uncoloured, as was the anterior aspect of the elbow, the forearm, and the hand, with the exception of a large blotch which lapped over from the posterior surface of the arm to the anterior on the radial side. The dorsal surface of the upper arms was deeply discoloured; as was also the dorsal surface of the forearm. The discolouration extended to a short distance on the back of the hand, and on its ulnar side to the root of the little finger. Between the roots of the fingers, on the dorsal aspect, there were large dark brown blotches. With this exception, the posterior surface of the hand was uncoloured. The soft palate was discoloured. On examining the chest, he satisfied himself that the percussion note was perfectly normal throughout. There was a total absence of precordial dulness. The action of the heart was regular, but exceedingly feeble, and unaccompanied with murmur or unusual sounds of any kind. Respiration was loud all over the chest, and accompanied with mucous râles in both cases.

On examining the abdomen he discovered enlargement of the liver, but the surface of that organ was smooth. No other evidence of organic disease was discovered. Judging from the appearance of the patient, from the peculiar symptoms she presented, both positive and negative, as well as from the condition of the blood, an examination of which by the microscope showed a great preponderance of the white corpuscles, the red corpuscles cohering in rolls, he made the diagnosis of Addison's disease, notwithstanding that the discolouration was partial. The patient's condition continued much the same until the 19th October. From that date she continued to improve up to the 1st of November. She was able to take food and drink; to sleep, and was in every respect much better; the diarrhea had been checked, and the vomiting had ceased. On the 2nd of November he found on visiting the hospital that the patient had that morning, and before she had taken her breakfast, vomited up about a pint of white grumous matter. This was repeated on the 4th of November in less quantity; and on the evening of that day

he discovered, for the first time, intermittence of the heart's action, and likewise of the pulse. The patient's tongue, which had been hitherto moist, became dry and glazed looking, as likewise the soft palate. She was now unable to take what she had been accustomed to use, port wine. Strange to say, however, she desired to have some porter this day, and retained it on her stomach, as she did several days afterwards. On the 11th of November aphthæ appeared on the edges of the tongue, and on the palate. On the 16th the pulse was down to 66, and intermitting.

On the 19th November she had a slight attack of syncope, and later in the evening was attacked with excruciating pain in the abdomen, causing her to cry most piteously during the night; and on the following morning he found her lying in bed with her legs drawn up, as if suffering from peritonitis. The abdomen was exquisitely tender, but there was neither flatus nor fluid distention. She was in a semi-comatose state, but when spoken to answered rationally and collectedly. The tongue was still glazed and dry. On the 27th November she had a slight convulsive attack, and twitching of the features; and two hours afterwards died in a state of syncope. The examination of the body, made a few hours after death, presented the following appearance:—The abdomen was covered with a layer of fat of very considerable depth. There was no wasting whatever; the body was that of a woman in more than average good condition for her class and years. In the abdominal cavity he found the peritoneum perfectly healthy. The omentum, the mesentery, and the retro-peritoneal tissue were loaded with firm yellow fat. The liver was considerably larger than it should be, but otherwise was healthy, except that its capsule was somewhat opaque and thickened.

The gall bladder was distended with bile, which was not to be wondered at, considering that for three days before her death the patient had taken no food whatever. The spleen was healthy in all respects. The stomach was very much reduced in size, being contracted, as well as the intestinal canal, owing to the patient not having taken food for so long a period. The mucous membrane of the stomach was slightly vascular, but free from abrasion, and otherwise in a perfectly healthy state. The kidneys were larger than they commonly are, especially the left kidney. An examination of this, on section, showed fatty disease in a very advanced stage. The cortex of the kidney seemed to be replaced by fat. The supra-renal capsule on that side was considerably enlarged. It was three times the average size of this body in the healthy adult. It was mottled on the surface. In the centre, or medullary portion, which should be void, there was a deposit of lardy-looking fatty material, which on examination with the microscope, was found to be pure fat. The cortex was occupied by a more solid and yellowish matter, which was found to possess all the characters of tubercle, consisting of fat-molecules,

which, on solution by sulphuric ether, left an amorphous granular stratum with imperfect cells.

The left semi-lunar ganglion was enlarged and sent off to the corresponding capsule several large nerve-filaments, which coursed over, and were then lost in the substance of the capsule. The right kidney was not so large as the left, but it presented also fatty disease. The capsule was scarcely so large as the left, but in section it presented an appearance precisely similar to it, except that the medullary, as well as the cortical portion, was occupied by a yellow tubercular-looking matter. There was a deposit of fat upon the surface of the heart, but the valves were healthy, except the mitral and tricuspid, which were a little thickened, but still quite competent to close the respective orifices. The lungs were found partially emphysematous. In the right lung some tubercle existed, disseminated through the organ, which was firmly attached to the chest-wall, and it became torn in process of removal. The left was likewise adherent, but less firmly, to the thoracic walls. The patient, two years previously, had complained of severe pain in the right scapular region, where the lung was found so strongly attached to the costal pleura. The number of tubercular nodules was greater in this situation than elsewhere, and some of them were in the stage of softening.

This case presented some features of interest because exceptional. In the first place, this was a case of partial discolouration only. He knew of only one other case of this kind where the discolouration was partial, to the extent of the lower extremities being entirely uncoloured, and where a *post mortem* examination showed disease of the capsules. That was a case recently reported by Mr. Holt in the *Lancet*, and reprinted in the *Medical Press* on the 1st of November last. In Dr. Hughes' case, and the one which he (Dr. Hayden) had lately brought before the society, there was complete discolouration. It illustrated the connexion between the tubercular diathesis and disorganization of the supra-renal capsule, and went to prove, what he had elsewhere ventured to urge, namely, that irritability of the stomach and discolouration of the skin were invariably associated with disorganization of *both* supra-renal capsules. It also illustrated what he had before remarked, that the left capsule was generally in a more advanced state of disease than the right. Lastly, the enlargement of the nerves sent from the semilunar ganglion to the diseased capsule, tended to support the views of Dr. Habershon, who holds that the irritability of the stomach and the disease of the supra-renal capsules are placed in the relationship of effect and cause through the hypertrophied connecting nerves. There was nothing in the stomach itself to explain such obstinate irritability. The brain was healthy, as were likewise the pelvic organs. It should have been observed that the urine was examined shortly after this patient's

admission—it was 1019 sp. gr., acid in reaction, and contained a good deal of albumen.—*December 2nd, 1865.*

Enchondroma.—MR. PORTER exhibited a specimen of enchondroma, or cartilaginous tumour, which was developed on the index finger of the left hand of a woman aged thirty-seven. It commenced on the inside of the finger six years ago, and gradually enlarged until it had attained a very considerable size. Its growth was unattended with pain, and the skin was healthy and quite movable over the tumour. He amputated the finger between the first, or proximal phalanx, and the second. The surface of the morbid growth resembled cartilage in appearance and consistence, but the interior contained a good deal of osseous material, against which the knife grated on cutting into the tumour.—*December 2, 1865.*

Ovarian Tumour.—MR. TUFNELL said the case of ovarian tumour which he was about to bring under the notice of the society might be of interest to the surgeon in a practical point of view, as few who saw the enormous size of the tumour which he now exhibited could imagine that the question could arise as to whether ovariectomy should be attempted or not. Any one who saw the solid mass upon the table, which weighed nineteen pounds, would think there could be no doubt upon the subject; yet, so masked was it by the accompanying symptoms that there was considerable difficulty in deciding not to operate, especially as the woman was constantly entreating to be delivered from the tumour. The patient from whom it was taken was thirty-eight years of age, married, and the mother of ten children. She was admitted for the first time into the City of Dublin Hospital on the 11th of July last. She stated that she had enjoyed very good health until about four years before, when she first noticed a hard small tumour, which she could grasp in her hand, in the right iliac region. This gave her little or no inconvenience until a year since, when the abdomen began to swell, and anasarca of the feet to follow. Upon admission into hospital in July she had all the symptoms of confirmed ovarian dropsy. The walls of the abdomen were tense; percussion in the anterior and lateral portions yielded a dull sound, while in the lumbar region only was there any resonance; and here, though less dull, it was not clear. The body was wasted, and the upright position obliged to be maintained in consequence of dyspnea from pressure upon the diaphragm. The abdomen was more tense in some parts than others. Thus, from the umbilicus downwards, and especially towards the left iliac fossa, this state obtained, and to such a degree centrally, as to necessitate the operation of tapping being performed two inches above the navel instead of in the usual situation. Paracentesis was demanded in consequence of the extreme difficulty of breathing. Twenty quarts of thick, glairy, brownish-coloured fluid of an extremely

viscid character were drawn off, so thick, indeed, that though the tube of the canula was unusually large, it took upwards of an hour to run off, and at last failed to flow altogether, although the cyst was manifestly not empty; the latter portion of the fluid hanging in a thick gelatinous string out of the mouth of the instrument. No inconvenience followed the operation, and she left the hospital upon the 29th of July, the case being regarded as one unfit for operation, and the patient then satisfied with the relief which she had obtained by the tapping. Upon the 4th of September she returned to the hospital, the abdomen being again distended to a considerable degree; but it was not until the 20th of October, or just three months from the preceding operation, that paracentesis became absolutely necessary. She was now tapped, and twenty-four quarts of fluid drawn off—this time of much thinner character and lighter colour; so different, indeed, was its degree of fluidity that through the same canula as before employed, the quantity named (twenty-four quarts), was run off in less than twenty minutes. This tapping took place through the same spot as the preceding one, viz., two inches above the umbilicus, and was followed by an attack of circumscribed peritonitis. She left the hospital, however, upon the 1st of November. Upon the 9th of November she returned, for the third time, presenting similar appearances as before, with the exception that the veins on the anterior surface of the abdomen were much more turgid. The girth of the belly at the umbilicus was now fifty-two inches; and the measurements from the pubes to the navel, one foot, and from the anterior superior spinous process to the navel, eighteen inches. The parietes in the pubic region were infiltrated with serum, and so cedematous that they descended between the thighs like a dew-lap. The inferior extremities were anasarcaous throughout, the left one more so than the right. Dyspnea was now so urgent that she was again tapped, and nineteen quarts of fluid drawn off; this secretion being thinner even than the last—of a colour resembling thin port wine and water. Its specific gravity 1013; neutral in reaction, and coagulating firmly both by heat and nitric acid. This tapping afforded relief, but not such as before, for she was now suffering also from dyspnea, in consequence of effusion into the right pleural cavity; the chest upon this side was universally dull; slight hemoptoe was also present; some blood too was passed by the bowels, which were obstinately costive. After suffering greatly from this combination of disease, she died upon the 27th of November—up to the last praying to have the tumour removed.

The *post mortem* examination, which was made seventeen hours after decease, afforded the following results:—The upper extremities and chest were greatly emaciated; the belly protuberant, being forty-seven inches round the navel; and the legs and thighs anasarcaous. The peritoneum contained a considerable quantity of straw-coloured fluid. The ovarian

tumour presented a pearly white glistening appearance, with very large vessels permeating its surface from below upwards. The abdominal walls were adherent to the front of the cyst for the space of six inches; below this appeared a flattened dense surface the size of a cheese-plate, and looking much like pancreas flattened out; below this a solid or semi-solid mass filling both iliac fossæ, but especially the left, the abdominal walls being adherent to it, and requiring much force to separate them. Firm adhesions existed also in both hypochondriac regions, whilst the transverse colon and omentum were so completely agglutinated to the tumour that the vessels inosculated. In the lumbar regions adhesions also existed, and the small intestines were united to the tumour in spots. The uterus was normal in size, and healthy, as was also the left ovary. The right ovary merged into the tumour, and was insensibly lost. The pedicle was about two inches long, and not thicker than an umbilical cord. It contained two sets of vessels, and was diaphanous in the centre. The weight of the tumour after removal and the emptying of the large cyst of its contents, was nineteen pounds. The intestines were healthy; the liver large and fatty, and very pale, with the gall-bladder empty. The kidneys were both perfectly sound. The urine during life had been scanty, and loaded with lithates; its specific gravity being 1035.

The chest exhibited marked hydrothorax on the right side. The pleural cavity was filled to distention with thin bloody serum; and the lung compressed into one-seventh of its natural size; when cut into, it presented numerous tubercles in the miliary form. There was no sign of pleural inflammation or exudation of lymph. The heart was pale, and there was some effusion of serum into the pericardial sac. Dr. Barker having examined a portion of the tumour under the microscope, said that it exhibited a cheesy, albuminous aspect without structure, a few blood discs, and fat cells.—*December 2, 1865.*

Fibrous Tumour.—MR. COLLIS exhibited a fibrous tumour which he considered of interest from the unusual locality in which it grew. Mr. Paget, in his work on tumours, states that unless in connexion with the periosteum of the jaw fibrous tumours are not often found in connexion with bone or periosteum. The specimen which he now presented was removed on Wednesday last from the upper arm of an elderly woman. It sprung from the periosteum, in the situation of the insertion of the deltoid muscle; and it possessed interest not only from the situation in which it was found, but also as regarded the question of prognosis. Mr. Paget said, that while fibrous tumours, as a rule, are innocent, still examples are found, occasionally, which are truly malignant, though not differing in structure in any appreciable manner from those that are innocent. Mr. Paget gave instances of three such tumours occurring in different situations of the body, and Mr. Collis did not doubt that by

careful observation instances of others might be found. It was of immense importance that the surgeon should be able to make some tolerable guess as to whether such a tumour as this, in such a locality, were innocent or not. There were three points in this case which led him to conclude that the tumour was possibly of a malignant character. In the first place the attachment of the tumour to the skin was extremely close; the skin had all that dimpled, puckered appearance generally observed on a scirrhus tumour; the second point was, that this tumour did not appear to be in any way encysted or cut off from the surrounding tissues, as occurs in fibrous tumours of the uterus. In this case the tumour ran into the neighbouring tissues, after the fashion of malignant disease, whether cancerous or not. The coraco-brachialis, triceps, and probably a portion of the biceps and deltoid were encroached upon by the morbid deposit. The periosteum was immensely thickened and very adherent to the bone; the latter, however, did not seem diseased. The third point which led Mr. Collis to conclude the tumour to be malignant was the character of the pain. All tumours connected with the bone will be more or less painful, and the pain will be greater at one time than another; but the patient in the present case described the pain as of a shooting, lancinating character, and this, taken in connexion with the other features of the case, would help in the diagnosis as to whether the disease was malignant or not. He had removed the whole of the tumour, and laid the bone bare, and it would now be a matter of considerable interest to watch the result. He therefore now put the case on record in order that hereafter the Society might have an opportunity of testing the prognosis by its future progress.—*December 2, 1865.*

Rupture of the Aorta.—DR. G. F. DUFFEY, Assistant-Surgeon to the 24th Regiment, communicated the following case, and laid the recent specimen before the Meeting.—The specimen which I have the honour of laying before the society was taken from the body of a soldier in the 24th Regiment, whose case is briefly as follows :—

He was thirty years of age, strong, and apparently healthy, but of rather a sallow complexion; he had eight years service, was married, and of temperate habits. He had an attack of rheumatism in 1862, and there was a small cicatrix of a venereal sore on the prepuce, showing that he must have had syphilis prior to enlistment, as there was no admission for that disease against him in his medical history sheet.

He was in the regimental hospital comparatively recently (during the months of July and August) for an injury of the right ankle, but was discharged on the 21st of August. Neither during the time he was in hospital, or at any other period, did he complain to the surgeon of the regiment or myself, of anything being the matter with him except the sprained ankle; but his widow states that he had complained to her of

pain in the left side of his chest for the last two and a-half years, and that during the week preceding his death this pain grew worse, his appetite failed, and he slept badly.

Still, however, he did not become sick; and up to the moment of his death was actively engaged with his duties. He was in the act of pumping water when he suddenly fell backwards; some of his comrades immediately ran to his assistance, and, seeing his congested face and protruded eyeballs, thinking he was in an apoplectic fit, poured some water over him, and carried him to the hospital (a distance of only eighty yards from where he fell) where the Deputy Inspector-General of Hospitals, Staff Assistant-Surgeons Maunsell and Thompson, and myself were at the time. He was then quite collapsed; his face was pale, and there was no pulse perceptible. He gasped two or three times, and expired within five minutes from the time he fell.

On making the *post mortem* examination I found that the pericardium was filled with clotted blood, the weight of which was nine ounces. The aorta was greatly dilated, but there was no distinct aneurismal sac. Its coats in some places felt thick and hard, and in others very thin and soft. On removing the areolar tissue its surface appeared greatly congested, and in some places yellow, cheesy-like patches were observed. On slitting up the vessel extensive and thick deposits of atheroma were seen; in some places the internal and middle coats were ulcerated, and in others little pouches, with excessively thin parietes, projected from the sides of the artery. Great difficulty was experienced in finding the rupture through which the blood had escaped into the pericardium. At last, however, after a long search, in which I was assisted by my friend, Dr. Bennett, we found a minute capillary opening leading into the pericardium from a little above the sacculus of the left sinus of Valsalva; the heart was perfectly normal.

This case, sir, I thought might be interesting to the society on account of the large extent of disease, which must have existed for a long time, and which led to so sudden and fatal a termination, being present without the coexistence of any important or acute symptoms to call the physician's attention to it. The position of the rupture is also, I believe, unusual. Arterial diseases should be particularly interesting to the army medical officer, as from the vagueness of the symptoms occasionally accompanying them, especially aneurism of the thoracic aorta, which, as Professor Stokes has remarked, may be one of the most latent of diseases, a man otherwise appearing, as this man did, in good health might be considered as a malingerer, and a fatal termination hastened by the performance of too arduous duties.—*December 2, 1865.*

Disease of the Mitral Valve, &c.—DR. LAW presented to the society a specimen of mitral, combined with aortic valve disease, the condition of

the former admitting of free regurgitation. The patient, a servant, aged twenty-one, was admitted into Sir Patrick Dun's Hospital, on the 12th of August, complaining of extreme oppression of breathing and of palpitation of the heart; he had general dropsy, his limbs being enormously swollen and his face very much puffed. The history he gave of himself was, that three months previously he had been ill from oppressed breathing, and had the same swelling he now complained of, but became better, and continued so until ten days before his admission to hospital. On examining him there was a strong double murmur heard behind the sternum, and also another single abnormal sound below the left mamma and external to it, and distinctly, too, between the scapulæ. He had the peculiar thrilling pulse of aortic valve disease, and there was increased dulness in the pericardial region. The hand placed over the heart discovered a distinct purring ("fremissement cataire") sound in that organ. On examining the patient in a sitting posture there was dulness on percussion on the left side of the chest inferiorly, and here there was no respiration heard. The man was rather unwieldy from his dropsical condition, but he was induced to go on his hands and knees, when it was found that where the chest was dull before it became clear, and where the respiration was before inaudible it was now distinctly heard. The diagnosis was regurgitant mitral valve disease, with aortic valve disease and effusion into the left pleural cavity. The limbs, although very much swollen, did not pit on pressure, and his general condition led Dr. Law to suspect albuminuria; the urine on being heated exhibited albumen in considerable quantity. His case was nearly hopeless when he came into hospital, but he made a short temporary rally. Such patients are often very fidgetty and restless; it is often difficult to keep the clothes on them or keep them in bed. This man could not be induced to keep himself covered; he got up and exposed himself, and thus got fresh cold; the dyspnea became more urgent, and dulness was found on percussion, not only on the posterior left but on the posterior right side. He became gradually weaker, the action of the heart more feeble, the abnormal sounds less marked, the pulse lost its thrilling character, and he expired from asthenia.

The examination of the body showed extensive disease of all the aortic valves. They were thickened, and on their surface was a large fibrinous deposit; there seemed to be both interstitial and superficial deposit. The mitral valve was a good deal thickened, and some of the chordæ tendinæ were ruptured. The auriculo-ventricular opening was unusually large.

It might be a question whether the fremissement cataire was caused by the aortic or by the mitral valves; but the direction of it induced Dr. Law to connect it with the condition of the aortic valves. The heart weighed 24 ounces.

In reference to the complication of aortic and mitral valve disease he

had frequently observed that the symptoms of aortic valve disease predominated in some instances, and in others those of mitral valve disease; and he had, not unfrequently, observed that in the same case these symptoms alternated with each other.

A question had been addressed to Dr. Law, on the occasion of his making a communication on mitral valve disease admitting of free regurgitation, as to the murmur, if it were not a soft murmur? Several elements entered into the formation of these murmurs—the density of the fluid, the nature of the valvular disease where valvular disease existed, and the strength of the organ. Chlorosis was the type of the soft murmur. M. Beau tried the experiment of injecting water into a tube, and he heard a soft bruit de soufflet; he thickened the fluid and the bruit ceased. That was what was done in treating chlorosis. Treatment thickened the blood by increasing the proportion of red corpuscles in it, and, as in M. Beau's experiments, caused the bruit to disappear. The condition of the valvular lesion would also influence the peculiar bruit, as would also the dynamic energy of the heart. There was one instance, at the Meath Hospital, where a person who had heart disease came in with fever, and during the fever the murmur disappeared, but returned when the patient got well of the fever. Most murmurs either disappear altogether or, at least become very faint before death.—*December 9, 1865.*

Cirrhosis of the Lung.—DR. JENNINGS exhibited a specimen of this disease, taken from the body of a man, aged forty-five, a bricklayer, and exposed to great variations of weather and temperature in the exercise of his trade. About five years since he had been engaged in setting steam boilers, and then contracted what he described as a "severe cold," attended with profuse hemoptysis, which continued for three days, and in a less degree, for some time afterwards. After about a fortnight he resumed work, though still suffering from cough and expectoration, until his admission into hospital on the 16th September, 1864, at which time his cough was constant and distressing, and attended with muco-purulent expectoration and hemoptysis. Much dyspnea and lividity of the face were also present. The physical symptoms were—general dulness of the right side, marked immobility during respiration, inability to lie on the left side or back. The heart was completely luxated to the right side, the apex being felt and seen beating regularly below the right nipple. The pulse was eighty, and regular; the skin cool. The right side of the thorax was unusually dull in front and behind, but less markedly so under the right clavicle, where loud pectoriloquy or bronchial respiration, with loud cavernous râles, and cavernous vocal resonance, were audible. The sounds were also distinct posteriorly along the right side of the spinal column.

The chest being exposed, the eye was instantly arrested by the drooping of the right shoulder, which, when measured, was found to be one inch lower than the opposite, and also by considerable sub-clavicular flattening. The measurement from the ensiform cartilage to the mesial line behind gave as the result, fifteen inches on the right and sixteen inches on the left side. Thoracic motion on the diseased side was scarcely, if indeed at all, perceptible. The left side was preternaturally resonant, and evidently was emphysematous. The expectoration was viscid, of a yellowish colour, and abundant.

The *post mortem* examination disclosed the following conditions:—On opening the thorax the left lung was seen transgressing the mesial line, and overlapping the heart, which was altogether displaced to the right side of the sternum. The right lung was converted into a small, hard, and tough mass, of a fibro-cartilaginous consistence. On making a section of it, the organ was seen traversed by widely dilated bronchial tubes, ending, even near the surface, in chambers and cavities of very considerable size, more than one of them being capable of containing a large nut. These latter cannot be mistaken for strumous vomicae, being lined by a dense and whitish membrane. The radiating lines of contractile cellular tissue are also strikingly displayed. Next after phthisis, one of the diseases with which cirrhosis of the lung is most likely to be confounded is, I need hardly say, carnification of the organ, the result of sthenic pleuritis which has terminated in absorption and subsequent ligation of the collapsed organ by the consolidation of the effused lymph. But yet in the great majority of cases, and where the patient has been seen at an early period, the different history of each, added to the distinctive physical symptoms peculiar to each, is sufficient to establish a diagnosis. In every case, however, after death, the pathological characters, of cirrhosis, namely, the obliteration of all traces of ultimate cell tissue, the dilatation of the bronchial tubes, the conversion of the organ into a tough fibro-cartilaginous substance, and its contracted volume, are sufficient to remove any doubt that may have existed during life.—*Dec. 9, 1865.*

Cirrhosis of the Lung.—*DR. FOOT* exhibited a specimen of cirrhosis of the left lung, taken from the body of a man aged twenty-nine, who died in the Meath Hospital of hemoptysis on the previous day. He had had pulmonary symptoms, dating from an attack of bronchitis two years ago, but no profuse hemorrhage from the chest previous to the fatal one, although his expectoration had been occasionally streaked with blood. The day before his death he passed blood from his bowels; the following day, while he was up and dressed, he was seized with a sudden flow of blood from the mouth, which ceased for about two hours, then recurred, and continued till he died from exhaustion.

On examining the body, its general colour was a blanched waxy-white; hypostatic congestion existed on the nates and posterior parts of the trunk to almost the usual degree. Devergie has denied the presence of this *post mortem* phenomenon in death from hemorrhage; but according to Casper it is present after every kind of death.. Cadaveric rigidity was very strongly marked.

On raising the sternum and costal cartilages adhesions were evident between either lung and these parts; the right lung seemed large and hypertrophied; it projected through the opening instead of collapsing; it also transgressed the median line towards the left side; the left lung was small, and retracted deeply into the left side of the chest., drawing the heart to a certain degree after it; the left side of the chest, viewed externally during life, had been comparatively flattened. Both lungs were adherent to the ribs and diaphragm, but the left especially, which it was necessary to cut out of the thorax; it was much reduced in size, firm and leathery to the feel, non-crepitant, and sank in water; on section it displayed the dilatations of the bronchial tubes characteristic of cirrhosis; and in its apex was a cavity, the size of an acorn, full of blood. The right lung was congested and emphysematous, enlarged in compensation for the contraction of its fellow, and bearing on its outer convex surface the impressions of the ribs; in its apex was a cavity the size of a walnut, containing a black coagulum—a string-like process of which passed into a branch of the superior division of the right bronchus; the trachea, glottis, and pharynx were occupied by a blackey and firm coagulum. In the stomach was a mass of coagulated blood, and about four ounces of thick tarry fluid; in various parts, also, of the small intestines thick pultaceous-looking blood was seen.

The specimen exhibited afforded a good example of the disease to which Dr. Corrigan gave the name of cirrhosis of the lung, and which has more recently been called bronchiëktasis. The want of harmony in volume of the two lungs, the altered position of the heart from concentric displacement, the limitation of the morbid process to one side of the chest, are pathological features of this disease. Its association with tubercular deposit, of which there are evidences in each lung, is unusual; it seriously complicates the diagnosis, and very probably accelerated the profuse hemoptysis which so suddenly terminated the case.—*December 9, 1865.*

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.^a
TWENTY-EIGHTH ANNUAL SESSION.

DR. M'CLINTOCK, President.

ADDRESS OF DR. CHURCHILL, PRESIDENT FOR THE PAST SESSION,
ON RESIGNING THE CHAIR.

GENTLEMEN OF THE OBSTETRICAL SOCIETY,—Before I resign this chair, in which your kindness placed me last Winter, it is my duty to lay before you a summary of the proceedings of the past session, so that at the beginning of another—the twenty-eighth—we may, as it were, count up our gains, and see where we stand.

The simple reading of the list of papers, or other contributions, will show that, as a society, we have not been idle; that the last session has added its quota of valuable information to that of former years. It will, I trust, also stimulate every member to contribute his share, be it ever so little, to our increasing store. If we cannot all make discoveries or educe general laws of primary importance, each in his sphere of observation may note with accuracy physiological or pathological facts, and the registry of such facts will be a valuable possession for our profession.

During the past—the twenty-seventh—session I find that, besides the opening, the society held six ordinary meetings, which were fully attended, and at which nineteen communications were contributed by the members; not always essays, but what are of equal value, specimens of malformation or disease.

Our indefatigable secretary, Dr. Kidd, read a valuable paper on dysmenorrhœa; Dr. Bennett exhibited a very remarkable specimen of dropsy of the Fallopian tubes; Dr. Cronyn exhibited a fetus, with congenital hernia cerebri, upon which Dr. Foot subsequently made an admirable report to the society; Dr. Cronyn also read a valuable paper on vaginal lithotomy.

Passing on to more strictly obstetric subjects, you will recollect Dr. Cronyn's instructive paper on rigidity of the os uteri as a cause of delay in the first stage of labour; Dr. M'Clintock's admirable investigations into the effect of ergot of rye upon the child; and Dr. Byrne's essay on fatty and hydatigenous degeneration of the placenta.

There were three communications of value connected with obstetric operations:—Dr. Kidd gave a very learned account of the induction of premature labour, and stated the result of his experience as to the value of fluid dilators; Dr. Halahan exhibited an ingenious perforator, of his own invention; and Dr. Telford showed to the society the uterus and

^a These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

placenta of a woman who had been delivered by version for placenta previa, and who died subsequently.

Three admirable papers were read on diseases or accidents following labour. For example :—Dr. Denham related five cases of tetanus, and a case in which an attack of laryngitis proved fatal after delivery ; Dr. Banon gave us the results of his operations for the cure of vesico-vaginal fistula, showing an unusual amount of success ; and Dr. Lalor contributed a most valuable paper on puerperal insanity.

We were indebted to Professor Haughton for a report of his visit to the lying-in hospital of Moscow ; to Dr. de Ricci for a report upon a treatise of Dr. Bribosia on the use of opium ; and also to the same gentleman for a communication from Dr. Purdon on the use of the bisulphites.

There was only one paper on infantile diseases, and that was from the untiring pen of our learned colleague, Dr. Henry Kennedy, on hydrocephalus.

Gentlemen,—As most of these papers have been, or will be, published in the journals for those who wish to consult them, I feel that it would be trespassing too much upon your patience were I to enter into a detailed analysis of them. You know their value, and will have no hesitation in agreeing with me that the past session is not the least successful of our many successful ones.

If no brilliant discovery was announced, a number of important facts, and a great amount of valuable, because personal, experience were put on record, and added to that store upon which scientific progress depends.

No doubt can exist, then, that the past session has been a step in advance, and to many, advancing by one or two steps at a time, seems very slow progress. Yet it is not really so ; and as years pass over, and we compare the past with the present, we are better able to estimate the advance which the combined labour of many workers has secured.

Most of our present members are too young to remember the formation of this society, but I and some few present assisted at its birth ; and if we take the twenty-seven years it has existed, adding thereto a few years, still within my own recollection, and review the continuous development of our branch of the profession, we shall find not only that a remarkable progress has been made, but that it is pretty much embraced within that period.

It may not be without both interest and instruction if we take a cursory glance at the improvements which have taken place in that space of time, say about thirty or thirty-five years, and I will try to be as little tedious as I can. I feel the more pleasure in doing this because, as I believe, the first move in the right direction came from the Dublin School of Midwifery ; and, moreover, that this hospital and this society have contributed a very large portion of the “ materiel ” upon which this advance has been based.

As the most important and influential means for advancing obstetric science, I reckon the existence of such an hospital as this, under whose hospitable roof we are now assembled. The enormous number of cases therein attended year after year, affords sufficient ground for general deductions; the many exceptional cases tend to define the limits of these general conclusions; whilst the great practical experience and the matured judgment of those who hold charge for so many years, enable them not merely to give sound instruction to the younger members, but on a more extended scale, and with a larger class, to benefit and enlighten the entire profession. In proof of this, I have only to refer you to the standard works of Collins, E. Kennedy, M'Clintock and Hardy, Sinclair and Johnston, together with the valuable monographs published in the past years by the masters and assistants of the hospital. The publication of such extensive statistics, although not a novelty on the continent, was so in Great Britain; partly, at least, because even now there is no other hospital in Great Britain as large. But of late years various contributions from England and Scotland of the same kind, and notably one from Dr. Hall Davis, have appeared, and we add them, and hope each year to add more to the extensive series we already possess. But whilst I claim for these extensive statistics very great value, it must not be supposed that all our different questions can be settled by them. At the most, conclusions drawn from them are but approximations—often very close approximations—to the truth. In many cases this is sufficient for our purpose, and when it is not quite satisfactory it may be the best at our command. We should neither undervalue nor over-estimate the deductions from statistical tables.

The next most important agent of progress, I think, has been this society. Whether the very valuable reports to which I have alluded would all have been published but for the stimulus afforded by this society no one can say; only one appeared previous to its formation; but assuredly the greater number of the valuable monographs founded upon them would not. It is when men engaged in similar pursuits are brought together in this way that individuals are encouraged and stimulated to submit their experience to each other. What might have remained vague floating opinions, under this test assume a definite expression, and are subject to careful consideration before submitting them to such a tribunal. "As iron sharpeneth iron, so the countenance of a man his friend by hearty counsel," and we have all experienced the personal benefit which the intercommunion of such a society as this affords.

It must have afforded high gratification to Dr. Evory Kennedy, the founder of this society, to see such a recognition of its value as was afforded by the formation of similar societies in London and Edinburgh. As in our own case, we can see the benefit which has resulted in the more

rapid progress of our science, and in the accumulation of published records of that progress.

The last agency I shall mention as having contributed to the advance of science is of a different character altogether, and yet deserving of being so considered, I think. I allude to the use of anesthetics, and more particularly to Sir James Simpson's memorable discovery of chloroform, for this purpose. I do not allude to its value in the prevention of pain in ordinary cases; but if you consider the facility it gives to the operator, by the absence of struggle—to the prevention of shock, which is so dangerous an element in all serious obstetric operations—and finally, to the facility it affords for a minute and otherwise painful investigation of various cases—you cannot doubt that it has powerfully aided in the acquisition of knowledge.

Well, then, we have our hospital as a source of extended experience; as an obstetric school for the young; and by its minute, ample, and classified statistics, affording to those more advanced data for the solution of various difficult problems; so, on a smaller scale, have Edinburgh, London, Glasgow, and many of the provincial towns in England, who have followed our example also in publishing statistical reports. Again, as a stimulus and a record, we have our Obstetrical Society, and so have Edinburgh and London; and it is probably due to this increased activity that so many valuable works, whether systematic or of a more limited range, have been published in addition to the statistical reports.

With these agencies actively at work, it may naturally be asked whether we can show corresponding results? Whether there has been any marked increase to our knowledge—any true scientific practical advancement? To enter fully into this question would occupy more time than I can command, and would be a greater strain on your patience than I have any right to exercise, but, I hope to be able to show, in a cursory notice, that such is the case.

Let us first speak of midwifery and then of diseases of women.

1. The anatomy and physiology of the organs concerned in generation and parturition have received a minute and thorough investigation, and their relations to the fetus, mechanically and vitally, have been established more precisely than formerly, and, consequently, natural labour has been recognized as a physiological process—as natural to this system as digestion to the gastro-intestinal apparatus. The inevitable effect of taking this view has been a greater reliance upon the natural powers, and an abstinence from all interference when these powers are adequate to their work.

It is not more than thirty years, I think, since Nægèlé's work was translated into English, and proved a starting point for those investigations, with the mechanism of parturition, which have solved nearly all the questions connected with the subject.

The use of the stethoscope, not only for ascertaining the existence of pregnancy, but the life, sinking, weakness, and death of the fetus comes, I think, very nearly, within the same period. Among the earliest labourers in the field were the late Dr. J. C. Fergusson and Dr. Evory Kennedy, and they left little more for their successors than the clearing up of some doubtful points and the more extended application of their observations.

Again, within this same thirty years, has been developed an increased appreciation of the difference between the first and second stages of labour, as to their respective effects upon the constitution of the mother and the well-being of the child; influencing the practical treatment in natural labour, but far more essentially in prolonged labour, according as the delay is in the first or second stage.

The management of the third stage of labour has undergone little change in Dublin since I was a student; but it seems to be undergoing a modification elsewhere, which consists in the adoption of the Dublin plan of steady pressure and traction by the funis. We all know that this pressure often extrudes the placenta without further assistance, yet this has lately been announced as a discovery by a German accoucheur. I think, also, that it is less usual now than formerly to wait the regulation half hour after the birth of the child, and it has been found that the sooner the afterbirth is away the less the danger of hemorrhage.

2. So much for a few of the points connected with natural labour, in which we must recognize an advance upon former knowledge.

But if the powers and agencies of natural labour be better known and its limits more accurately mapped out, it follows, as a matter of course, that great light must be thrown upon unnatural labour considered as a pathological process. The two leading considerations which determine our practice in deviations from natural labour are now admitted to be:—1, the excessive duration of the second stage; and 2, the bulk of the child as compared with the dimensions of the passages it has to traverse. Upon the fuller recognition of these facts, and their consequences, has been based all the improvements in the management of such cases.

For example, there are certain obstructions which impede the progress of labour; if these only act upon the first stage, having determined that there is no danger in that to mother or child, we endeavour to remove the obstacle, but we do not interfere with the labour.

But let the obstruction be of a permanent kind so as to act upon the second stage, or let there be a want of power entailing an unlimited prolongation of that stage, and we know at once and beforehand that we must interfere, sooner or later, and the exact time will be determined by a comparison of the condition of the mother and the state of the child as ascertained by auscultation. The definiteness of our knowledge has led to more distinct practical aims than were to be found in the teaching or writing of former times.

The mode of interference will be decided chiefly by the amount of obstruction, and here I arrive at what may well be considered as the most beneficial change I have to notice. The stethoscope in enabling us to ascertain that the child is alive has really increased our estimation of the value of fetal life. Knowing the child to be alive, we have no right either to kill it or let it die, if by any means it can be saved without destroying the mother. Thus in all, or nearly all, those cases in which the defect is a want of power, and in those where the obstruction is moderate (the child being alive), the use of the forceps has superseded both delay and the use of destructive instruments—to the great saving of infant life. Any one who takes the trouble to compare the first and last report of this hospital cannot but be struck by the difference. A similar change has taken place in Scotland and England, and the rule is now definitely laid down in all recent systems of midwifery. In this country, at least, the credit of the change is primarily due to the late Dr. Beatty, and next to his son, Dr. Thos. Beatty; we may say that the substitution of the forceps for craniotomy, is a decided improvement which has been rendered safe by chloroform and valid by the stethoscope.

Another suggestion, based upon the endeavour to save infantile life, is the substitution of version for craniotomy in certain cases of deformed pelvis. Its proposer, Dr. Simpson, is very sanguine of its extended success, and no doubt that when the deformity is not too great it is of great practical value.

Favourable accounts have been published of the value of the cranioclast and cephalotribe in facilitating the extraction of the mutilated fetus, and so far increasing the safety of the mother, but further experience seems necessary before arriving at any decided conclusion.

Lastly, there are two alternative operations—induction of premature labour, and the Cesarean section, upon which I must say a few words. The first has been rendered, both easier and safer, by the use of the water douche, first proposed by Kiwisch, of Prague. It can also be employed in cases where any other means would be extremely difficult—this is a clear gain of modern times. I think there is ground of hope that our improved mode of treating incised wounds of the peritoneum, as evidenced in the increased success of ovariectomy, will tell upon the operation of hysterotomy, and that a diminished death-rate, from this fearful operation, may be the result. In which case it may, probably, be substituted for craniotomy in extreme cases.

3. The third class, into which I have divided the improvements under consideration, embraces certain complications irrespective of the kind of labour.

Here I may adduce the use of chloroform in epileptic convulsions, which confers a twofold benefit; it not only enables us to effect the delivery promptly and without pain, but it will arrest the convulsions.

Again, we turn with pride and hope to the removal of that opprobrium of surgery—the treatment of vesico-vaginal fistula. Now by the aid of chloroform, the employment of metallic sutures, and the use of improved instruments; it has been established that these cases are not beyond cure. Let me add the hope that the earlier termination of labour by art will, in future, very much reduce the number of these cases.

Further, modern researches have thrown some light upon that terrible scourge of childbed—puerperal fever. Not that we are in possession of the means of prevention or cure, but the general principles which regulate our treatment of other epidemics have been applied to this, and the recognition of the change of type has led to a corresponding change of treatment. The essential character of the worst form has been more clearly established, whilst, at the same time, our knowledge of the local complications has been increased.

Lastly, modern observation has added to our list of puerperal diseases and accidents, some of which, a few years ago, were facts unknown. For example, in works of thirty years ago you will find no description of thrombus of the labia, paralysis in childbed, tetanus, embolism or arterial obstruction, not to mention those, still more obscure, which cause sudden death. Now they are familiar to all.

This, gentlemen, is a very imperfect sketch of some of the improvements in midwifery which appear to me to indicate undoubted progress in scientific knowledge and practice; and now, gentlemen, if I have not tired out your patience, let us turn for a few minutes to the subject of diseases of women, and see if that department has been equally progressive.

The minute anatomy of the unimpregnated organs, their relation to each other, and their position in the pelvis, have been more minutely investigated, and their physiological inter-dependence so far satisfactorily ascertained as to render an intelligent pathology possible. The line of demarcation, thus mapped out, all on the one side pertains to healthy action, all on the other is disease.

And the improved pathology of the present day has been applied to morbid conditions of the ovario-uterine system: reflex irritations, cell growth, depraved nutrition and its results, epithelial and cancerous growths—all find fitting illustrations and afford explanations of different conditions of this important system.

Practical treatment of the various diseases has also kept advancing, whether of a dynamic or mechanical kind. The latter, indeed, has attained so unusual a development that there seems to be some fear that the former may be forgotten. Instruments for dividing or dilating the cervix, for rectifying malpositions of the uterus, or for removing out-growths, are constantly multiplying, as you all know.

Much of all this is a decided gain, and may compare favourably with

the state of our knowledge and practice thirty years ago. But, perhaps, no test of progress in any branch of our profession is more certain than the accuracy to which the diagnosis of disease has attained. Now, I know of no department of our branch of the profession in which such advances have been made as in the diagnosis of disease. Formerly it depended mainly upon the comparison of symptoms with the results of external palpation and internal digital examination, and how much was left vague or unknown a very superficial acquaintance with books must have taught you.

All displacements but two, all intra-uterine growths, all the superficial diseases of the vagina and cervix could only be guessed at; whereas now they are so brought within our reach that they may be as accurately known as external diseases.

They had, and we have, four sources of information in common, viz., the collation of symptoms, and the evidence deducible from auditory, ocular, and tactile investigation. They understood fully the first of these, and the last, so far as the finger enabled them; but they had only just heard of the stethoscope and the speculum, and not at all of the uterine sound.

But now, we not only avail ourselves of the ordinary use of our eyes and fingers, but we have supplemented them by higher or more extended powers.

I remember well, in Paris, thirty-five years ago, seeing Recamier and Lisfranc use the speculum, which they had recently revived; but almost all the knowledge we have derived from it is of more recent growth, and is due to the labours of Evory Kennedy, Simpson, Bennett, Boivin and Dugès, Becquerel, Aran, Jobert, and others.

Let us inquire now *what* we have gained.

Formerly, leucorrhœa, or some equivalent phrase, was the term for all non-sanguineous discharges from the vagina, and it was regarded as resulting sometimes from inflammation, sometimes from debility. Now, every one is aware that the mucous membrane of the vagina and uterus is subject to the same variety of diseases as any other mucous membrane; that you may find simple or granular inflammation of the entire surface, or a part of it; that you may have papular, vesicular, or pustular eruptions in the vagina; or diphtheritic exudation, or excoriation, simple or malignant ulceration of the cervix, granular erosion of the cervical canal, and vascular tumours at the os uteri, as well as small polypi.

The use of the speculum, of one kind or another, renders the diagnosis of these affections easy and sure.

But our eyesight has been further supplemented by the microscope; and although our information is neither so complete nor so precise as it will be, yet enough has been added to our previous information as to the nature of growths and discharges to satisfy us that it will become of

great value in diagnosis. It is quite possible for a good microscopist to detect evidences of cancerous disease both in tissue and in discharges, and mainly by the aid of this instrument the diagnosis between epithelioma and cancer of the cervix uteri is established. In the domain of anatomy, I myself witnessed an instance of its value. You are aware, most of you, that one difficulty in the way of accepting the ovular theory of menstruation is that the number of Graafian vesicles in each ovary seems inadequate to supply one or more a month during the active period of uterine life. It has been suggested that the supply must be kept up by repeated secretion of them by the parenchyma of the ovary. But this is quite unnecessary. Some time ago, my friend Dr. Richardson, of whose great microscopic skill many of you are aware, was good enough to show me a minute portion of ovarian parenchyma, under a lens of very high power, and I saw in that small fragment more Graafian vesicles than by the naked eye you can see in the ovary; enough, and more than enough, to supply the necessities of menstruation and gestation, if the other portions be as richly endowed.

So far, however, our eyes have only explored the canal leading to the womb, and the os uteri, with products obtained therefrom. Of the visible condition of the uterine cavity we have known nothing but that such knowledge would be most valuable and desirable. Nor shall we be long without it. Owing to the recent improvements made in the endoscope by my friend and pupil, Dr. Cruise, a vivid light can now be thrown into this cavity, and I have more than once been indebted to him for a view of it which satisfied me that by this instrument we can at least determine the existence of unusual vascularity and granulation. I hope to have an opportunity of thoroughly investigating the subject, now that we have the means, and I confidently expect that we shall be able to complete our knowledge of the diseases of the internal surface of the uterus, as the speculum has enabled us to do for those of the vagina.

So much for the extension of the power of vision. The sense of touch has also been supplemented by the uterine sound. This long finger, as one may call it, was first introduced into practice by Dr. Simpson, in Great Britain, and M. Huguier, in Paris, without any communication with each other.

Its use is twofold—to penetrate where there is not room for the finger, and to extend beyond its reach. And the result has been the discovery of diseases and displacements previously unknown, and an increased precision given to our knowledge of those previously known.

For example:—By it we can ascertain the dimensions of the os uteri, and canal of the cervix, whether it be unusually narrow, strictured, or contorted, and the exact locality of these affections.

Again, although retroversion of the pregnant uterus has been recognized since the time of Wm. Hunter, no one suspected the existence of such a

displacement in the unimpregnated until it was revealed by the sound. The same may be said of anteversion and lateral flexion.

Moreover, the length of the cavity of the uterus can be measured by it, and so the nature of the uterine enlargement ascertained. The diagnosis of retarded involution is mainly due to the use of the sound.

According to the experience of M. Huguier, with two or more sounds we may take the measure of intra-uterine tumours or polypi, and certainly we can ascertain the mobility or comparative fixedness of the uterus, which in many cases is of extreme importance. It is notoriously difficult in many cases of moderate abdominal tumours to decide whether they are uterine or ovarian, yet this difficulty has been removed by the use of the sound.

So much for diagnosis. Now, if we turn to the treatment of this class of diseases we find equal evidences of improvement.

In the first place, the speculum, which reveals so much of which we should otherwise be ignorant, enables us also to apply our remedies exactly to the part affected, and to use many which would in no other way be available. You may now safely apply caustic solutions, strong caustic, or the actual cautery, with the certainty that the result will be limited to the part you wish.

Formerly, disk and ball pessaries were the only mechanical remedies against displacements; now I need not tell you that pessaries of various kinds, for the different displacements, have been multiplied, so that the puzzle is rather which to choose.

The cervix has been divided for stricture, or dilated by tents; nay, even fibrous tumours have been enucleated or destroyed. Intra-uterine polypi have been reached by the wire *écraseur*, or when they have descended have been removed by that instrument, instead of the tedious process of the ligature, and subsequent sloughing.

I have already alluded to the operation for vesico-vaginal and recto-vaginal fistula which have been so signally successful.

Lastly, it has remained for our times to witness the success of ovariectomy, and its reception into the domain of surgery.

Gentlemen,—Have I said enough to prove the advance of our branch of the profession within the last thirty years? I hope I have, though my sketch is very short and imperfect, partly from a fear of trespassing too much upon your good nature, and partly from want of leisure. If I have, my object has been attained, and I will conclude with two remarks, directed to the junior members of the society more especially.

First.—If we except the discovery or application of chloroform there is no great stride in all this progress. It has been made step by step, one leading to another. A better knowledge of the anatomy and pathology leading to more care in contriving aids for diagnosis and modes of treatment; a thoughtful consideration of what was wanting

for the detection and elucidation of diseases led to attempts at supplying such means, and a conviction of the imperfection of some led to additional trials until the result was attained. A feeling of the advantage of light, and the possibility of applying it, led to the speculum, and a consciousness of the knowledge that would be gained led to the uterine sound.

Secondly.—As our progress has been gradual, and not the result of brilliant discovery, so the men to whom it is due, with few exceptions, were not men of genius, but men very much like ourselves, with this peculiarity, that they were careful, thoughtful, observers; men who collected facts accurately, and reasoned from them judiciously; men who never spared themselves so that they might discover the truth. Not giants in intellect, though of cultivated minds; they were giants in working, and they “have left their footsteps on the sands of time.”

For us all, for the young emphatically, this is the grand secret of success. Work—not for mere money, though you will gain it; not for fame merely, though there is no other way to attain it; but work, because it is given you to do; work, because it is your duty to yourselves, your duty to the profession, your duty to the public, your duty to God.—18th Nov., 1865.

DR. M'CLINTOCK read the first part of a paper on Rupture of the Vagina, which will be published when completed.—Dec. 9, 1865.

On the Treatment of Puerperal Fever.—DR. TELFORD read the following paper:—

I purpose to bring under the notice of this society some cases of well marked puerperal fever, occurring in the Lying-in Hospital during the last year, treated by tincture of aconite and anodynes, with the free administration of stimulants.

CASE I.—Margaret Hernan, aged forty-four, a strong healthy-looking woman, was admitted to the Dublin Lying-in Hospital on the 17th of March, 1865, having been confined of her thirteenth child early on that morning on board one of the steamers coming from Liverpool. Her labour was quite natural, and the child healthy. March 22nd, her fifth day after delivery, she had a severe rigor, and complained of headache; the pulse was 130, tongue furred, abdomen tender on pressure, lochia suppressed, and the breasts soft and flabby. Ordered a turpentine stupe to the abdomen, to be followed by a large poultice of bran, and to have one of the following draughts and powders alternately every second hour:—
℞. Tinct. aconite (Fleming), M V; liq. opii. sed. (Battley's), M X; etheri. chlor., MXX; aqua ad 1 oz. M. ℞. Pulv. ipecac. lo., gr. 5; hyd. c. creta, gr. 2. M. She was also allowed 4 oz. of wine and some arrowroot, with full anodyne at bed-time.

23rd, 10 a.m.—Slept some during the night; still complains of pain

over the abdomen; lies on her back with the knees drawn up; the face has a peculiar congested appearance, which I have often noticed in cases of puerperal fever; pulse 125; bowels confined. Ordered a dose of castor oil and tinct. of senna; to have her draughts repeated; 4 oz. of wine, arrowroot, and chicken broth.

24th.—Is this morning much troubled with constant vomiting, for which two drops of hydrocyanic acid were added to each of her draughts, and 4 oz. of brandy given.

Evening, 7 p.m.—Seems very low and weak; complains much of flatulency, restlessness, and extreme thirst; tongue dry and brown; pulse 120. Ordered turpentine enema, full anodyne, and 6 oz. of brandy.

25th.—Vomiting still continues; tympany rather increased; pain on pressure over the abdomen; draughts to be repeated; 6 oz. of wine, some ice, for the purpose of allaying thirst, and checking the vomiting, and to have an anodyne at bed-time.

26th.—Vomiting has ceased; other symptoms much the same; repeat medicines.

27th.—Seems something better; tympany much less; pulse 120. To continue her medicines, and have one ounce of the following mixture three times a day:—*R.* Tinct. ferri mur. dr. iii.; infusi quassiae, 8 oz. *M.*

30th.—Looks much better this morning; her expression greatly improved; tongue cleaning towards the edges; pulse 100.

April 3rd.—Is getting much stronger; draughts omitted. To continue the iron mixture. Convalescence from this time progressive; discharged April 17, just one month from admission.

CASE II.—Ellen Moseley, aged twenty-three, a delicate-looking woman, was confined of her first child November 2nd, 1865; her labour was natural. On the second day she complained of some pain over the uterus, which was preceded by a rigor; tongue clean and moist; pulse 120.

Nov. 5.—Pain on pressure greatly increased; lochia scanty and foul; no appearance of milk in the breasts; tongue still clean, pulse 140. Ordered the draughts and powders as in the first case, a stupe and poultice to the abdomen, and to have the vagina syringed with water containing some of Condy's disinfectant fluid.

Evening, 7 p.m.—Symptoms much the same, except the pulse, which had fallen ten beats in the minute. Ordered full anodyne, and two ounces of wine.

6th.—Pain on pressure still acute; tongue dry and brown; complains of great thirst, and profuse perspiration; pulse 130. To have her draughts repeated; wine 4 oz.; to be allowed some ice to suck; arrowroot, chicken broth, and jelly.

7th.—Continue medicines; symptoms much the same.

Evening, 7 p.m.—Patient very low; abdomen tympanitic. Ordered an anodyne, with half a drachm of spirits of turpentine; to have her poultice repeated, and to get 4 oz. of wine.

8th.—Complains very much of vomiting, which seems to distress her greatly; abdomen very tympanitic, and extremely tender in the left inguinal region; pulse 120. Ordered a blister to the abdomen, her draughts, with two drops of prussic acid added, a turpentine enema, and 4 oz. of brandy.

Evening.—Vomiting still continues; the vomited matter being ejected without any effort; the blister has risen slightly. Ordered a poultice to blistered surface; a full anodyne, with turpentine; 6 oz. of brandy, and a bottle of Seltzer water.

9th.—Vomiting stopped, and tenderness something less; pulse 120, tongue cleaning; draughts to be repeated; wine 4 oz.; bottle of Seltzer; and to have an ounce of the following mixture three times a day:—*R.* Tinct. mur. iron, dr. iii.; infusi quassia, 8 oz. *M.*

Evening.—Weak and faint; pulse 120. Ordered 6 oz. of wine, and full anodyne.

She now went on very well for some days, when she had a second rigor on the 17th of November, accompanied by tenderness over the abdomen, and a troublesome short cough. To have a poultice to the abdomen; her draughts repeated; 4 oz. of wine; and the following draught at bed-time:—*R.* Sol. mur. morphia, dr. i.; acid, hydrocyanic m. ii.; aquæ ad. 1 oz. *M.*

18th.—I found her something better in the morning; tenderness not so much complained of; abdomen tympanitic; pulse 120; medicines repeated, and 6 oz. of wine.

On the 20th she was attacked with troublesome diarrhea, for which she was ordered half-drachm doses of Belloc's charcoal, with five grains of Dover's powder, every three hours, and 6 oz. of wine. This treatment succeeded in checking the diarrhea. The only troublesome symptom which now remained was tympanitis; this also gradually subsided, and she is now quite convalescent.

CASE III.—Ellen Farrell, aged twenty-one, was confined of her first child November 20th, 1865; labour was quite natural, being completed in twenty-one hours. On the morning of her third day she had a rigor, which was attributed to the coming on of the milk, as the breasts were full and painful; the uterus was enlarged, and tenderness complained of on pressure; pulse 120. Ordered camphor and Dover's powder, of each five grains, three times a day, and to have a poultice over the uterus; anodyne at bed-time.

24th.—Pain on pressure on the abdomen very much increased; lochia suppressed; breasts still somewhat distended; face hot and flushed;

tongue furred; pulse 130. To have a turpentine stupe to the abdomen, followed by a poultice; one of the aconite draughts three times a day; 4 oz. of wine, and an anodyne at bed-time; arrow root and chicken broth.

26th.—Pain on percussion still continues; abdomen tympanitic; tongue covered with a white fur; pulse 120. To continue treatment, and have a turpentine enema, which was repeated for the next three days, owing to the continuance of the tympany.

30th.—Patient much improved; tongue cleaning; and the pulse much less frequent.

From this period the treatment consisted of the administration of tonics and nutritive food. She was discharged, quite well, December 8th.

CASE IV.—The case which I am now about to relate derives its chief interest from the masked character of the symptoms in the commencement of the disease. Mary Dillon, aged twenty-two, was confined of her first child November 5th, 1865. On the second day after delivery she complained of acute pain in the right hip and thigh, which was greatly relieved by an anodyne liniment.

8th.—Complains of some pain on pressure on the abdomen; skin hot and moist; tongue furred; and the pulse 120. Ordered the aconite draughts; stupe and poultice to abdomen; wine 4 oz.; and anodyne at bed-time.

9th, 10 a.m.—Pain of abdomen much diminished; pulse 110. Continue treatment. She now went on very well for four or five days, when she was removed to the convalescent ward, her symptoms then being those principally arising from weakness; the pulse was quick; skin hot; tongue slightly furred; and she took her food with some relish, and seemed very hopeful of herself. Ordered *R. Tinct. mur. iron, dr. iii.; infusi quassiaë, 8 oz. M.; 1 oz. three times a day; wine 4 oz.*

14th.—She had a smart attack of diarrhea, for which various astringents were ordered, and 8 oz. of wine. This treatment kept the diarrhea in comparative check till the 18th, when it set in in a much more severe form, with mucous stools, very much resembling those in dysentery; the stomach was irritable, but the administration of hydrocyanic acid checked this symptom; tongue coated, and the pulse varying between 130 and 140. Various plans of treatment were tried to check the diarrhea; solid opium, anodyne enema, acetate of lead, and opium, Belloe's charcoal, &c., &c., but all without effect. Brandy was also given largely.

On the 20th the abdomen became tympanitic; stools passed under her; the face assumed a peculiar pinched look; countenance very anxious; and the pulse very rapid and weak. She gradually sank, and died Nov. 23rd, the 18th day after her confinement. The last two days she was supported, I may say, altogether by brandy.

Autopsy ten hours after death.—The surface of the body was greatly emaciated. On opening the abdomen a large quantity of serous fluid, mixed with flakes of lymph, escaped. On further laying it open we were surprised to find the intestines matted together, and a large quantity of purulent matter in the lower part of the abdomen. The uterus was healthy, nor was there a trace of ulceration in the intestines, as we expected.

The details of these cases, and the favourable result of the practice adopted, viz., three recoveries out of four, would seem to prove that the administration of the tincture of aconite had a beneficial effect, from its action both as a sedative and anodyne, lowering as it does the heart's action without injuriously depressing the vital powers; occasionally it has an aperient action, which, however, does not appear to act detrimentally. Of course I need not tell the members of this society that a patient under the influence of aconite should be very carefully watched.—*9th Dec., 1865.*

Ovariectomy.—DR. P. C. SMYLY exhibited the injected, dried, and inflated ovary removed from the patient whose case is recorded at page 10.—*13th January, 1866.*

On the Use of Galvanic Pessaries.—DR. KIDD read the following paper, introducing it with some complimentary remarks as to the title conferred by Her Majesty on the inventor of the galvanic pessary:—

On the 26th January, 1853, Professor (now Sir James) Simpson read a short paper, before the Obstetrical Society of Edinburgh, on "Imperfect Development of the Uterus, giving rise to Amenorrhea, &c.," and stated that "he had seen, in some of these instances, the wearing of an intra-uterine galvanic or zinc and copper pessary, gradually increased in size, followed by the best results, and even occasionally by cure of the amenorrhea."

Having been induced by these observations to make use of galvanic pessaries in several cases, I have thought it well to relate to the society some of the results I have obtained. The first case I used them in was one of amenorrhea from imperfect development of the uterus, the imperfection being due rather to injudicious treatment at the commencement of puberty than to any congenital defect. When menstruation first began the patient of whom I speak got into a cold bath, which checked it immediately, and it did not again appear for more than twelve months. When it next occurred the lady, who was still very young, and not warned as to the care she should take, again checked it; and after this it recurred only at intervals of many months, till, her general health beginning to fail, she was placed under the care of the late Dr. Montgomery. Under the treatment prescribed by Dr. Montgomery, of which

horse exercise was an important part, the general health improved, and menstruation took place every second or third month.

I was consulted in April last. At this time the lady, who was about twenty-five years old, had been nearly two years married, but without any improvement as to the regularity of menstruation. Of a dark complexion, with a good figure, and well-developed bust, she had very excellent general health; and, living at the time in a hunting country, she was in the habit of accompanying her husband once or twice a-week in following the hounds. She told me menstruation seldom occurred oftener than once in two months, and when it did occur lasted only a few hours, and was so scanty in quantity as to scarcely stain her linen. On examination I found the mammae and external organs of generation well developed; but the uterus was small, the cervix small and conical, the fundus anteverted, the os so small that the smallest catheter could not be introduced. The ovaries I had reason to believe well formed, not only from the general good development, but from the circumstance that one of them lay in the recto-vaginal pouch of the peritoneum, and could be distinctly traced, of a natural size, by the finger placed in the vagina.

I confess I had very little hope of being able to induce regular menstruation in such a case. My first step was to dilate the os and explore the uterus, which I did by means of sea-tangle, but not without considerable difficulty, as even the smallest sized tent could not be introduced within it more than a few lines at a time, so that it was necessary to dilate it, bit by bit, introducing a small tent one day as far as it would go, and putting a sponge in the vagina to keep it from falling out; then passing another tent further the next day, till at the end of a week I got the whole canal of the cervix opened, and was able to introduce a sound, and explore the cavity. I was glad to find that though the whole uterus was small in size, the cavity was not much under the normal length.

I now kept the canal open by the frequent use of the sound, and had recourse to the usual treatment for amenorrhea. Among other things, I used warm hip-baths, sinapisms to the groins and mammae, aloetic purgatives, steel, ergot of rye, and a remedy that has been recently much vaunted in foreign journals—apiol; but the ill success of all may be judged of from the fact that the first and second menstrual periods after the treatment commenced had an interval of eight weeks and one day—that is to say, she came under treatment on 29th April, the period being then some time past, and the first menstruation was on the 30th of May, and the next on the 26th July, being exactly eight weeks and one day between the commencement of one period and that of the other. The discharge in both instances, though more than formerly, was very scanty and for fully a fortnight before it commenced she suffered from pain and swelling of her breasts and other symptoms of the menstrual molimina.

I now resolved to use the galvanic pessary when the next period was nearly due; and in the meantime I continued the steel, and warm hip-baths every night, desiring her to use a syringe while in the bath, so as to throw a stream of warm water into the vagina. The history of the following periods may be shown in a tabular statement:—

Menstruation expected	Pessary introduced	Menstruation occurred	—
First attendance, April 29, - -	—	—	
May —, - -	—	30	
July —, - -	—	26	Eight weeks and one day interval.
August 23, - -	15	17	
September 14, -	11	13	Profuse discharge, lasting eight days.
October 11, - -	12	15	Tried without pessary till one day past the time menstruation was due.
November 12, -	9	13	Profuse discharge, lasting eight days.
December 11, -	—	12	No pessary introduced.
January 9, - -	—	—	Ditto.

The last menstruation was due on the 9th inst., but has not appeared. When I saw her on that day I found the body of the uterus slightly enlarged, so that I could distinguish it above the pubes, and as there was no sign of the menstrual molimina, I thought it just possible that conception might have taken place after the last period, and I did not introduce the instrument. When I examined the uterus I found the cervix as large as in healthy women, and the os so large that I have no doubt the sound would have passed freely through it, had I been inclined to try it—and this at the expiration of two months after the use of any instrument.

The object in the management of this case in the latter months was to establish the habit, or “the memory of the body,” as Hunter calls it—and the question remains to be decided by time, as to whether that memory has been but of short duration, and that menstruation has failed for want of the memory having a refresher applied to it; or whether the organs have entered on still higher functions than merely menstruation.

During a visit to Edinburgh in September last, Sir James Simpson was so kind as to show me several cases in which he was using galvanic pessaries to stimulate the growth of ill-developed uteri; and it was

evident, from what I saw, that a very great degree of success had attended on their use.

The case now detailed establishes, I think, that the galvanic pessary will not only stimulate the growth of the uterus, but also that by its use the occurrence of menstruation may be determined at the proper periods.

In two other classes of cases I have tried this method of treatment: First, in cases of deficient involution of the uterus after labour. In some chronic cases of this kind I have tried it, with the hope that the continuous galvanic current would stimulate the processes by which the uterus is restored to its normal size, but without any beneficial effect.

The second class of cases I have tried it in is where chronic thickening of the uterine walls remains as the result of a sub-acute inflammatory condition occurring after labour. I have a case of this kind in hospital at present where this state existed eight months after delivery, and where the wearing of one of these pessaries for the last fortnight has greatly reduced the thickening and induration that had existed. The introduction of the pessary in this case caused a menstrual discharge, in the first instance, which had not taken place since her confinement, though she had not been nursing; though I did not remove the instrument because of the discharge, it ceased at the expiration of five days.

That a very considerable chemical and galvanic action goes on while the pessary is in the uterus is evident from the deposit found on the zinc portion of it when removed. This may be seen in one of the pessaries I exhibit, which I removed from a patient to-day, and present in the state in which I found it.

Another evidence of this action is afforded by a pessary that has been in use for a considerable period, the zinc portion of which is so corroded in one place as to have formed a perforation.

In some cases I have ordered a mixture of vinegar and water to be injected into the vagina when the instrument was in, with a view to increase this action; and when the instrument is long in use it is advisable to take it out occasionally to clean the surface of the zinc.

The pessary consists of a stem, half of which is of copper and the remainder of zinc. This stem is attached to an oval bulb, and the whole is made of a thin shell for lightness. The stem is placed within the uterus, the bulb remaining in the vagina, with the os uteri resting on it. The stem is made of different sizes or diameters, and about two inches and a quarter in length—that is, a quarter of an inch less than the normal length of the uterus, lest it should press injuriously on the fundus. For a case, where the uterus is shorter than usual, at present under my care, I have had the stem shortened in proportion to the length of the uterus, so that it may still be kept from pressing on the fundus.

In introducing the pessary a uterine sound is placed in an opening left in the bulb for the purpose; then, the sound being held in the right

hand, the left forefinger is placed against the os, and the pessary is run up on the finger, and with it guided into the os. When the uterus lies with its fundus well directed forwards the pessary will remain in without support; but when the uterus is more vertical, a disk pessary must be placed in the vagina to keep the galvanic pessary from falling out.

The patient experiences very little inconvenience from the presence of the instrument, and can pursue her ordinary avocations while wearing it. In some cases it may be worn for months; but when the object is to establish a regular habit of menstruation it should be introduced a few days before the time the discharge should occur, and be removed as soon as it appears.—13th January, 1866.

Medicated Pessaries.—DR. KIDD exhibited specimens of medicated pessaries and suppositories, and of the substances used for making them, and made the following remarks :—

I believe all who are engaged in the treatment of uterine disease admit the usefulness of keeping certain substances constantly applied to the diseased structures. Of the substances used, anodynes, astringents, and alteratives, such as the preparations of mercury and iodine, are most valuable, and a good mode of applying them is very desirable. Sir James Simpson suggested, many years ago, the use of “medicated pessaries,” in which a combination of lard and wax was used as the foundation with which the medicinal substances were incorporated, and made into balls that might be introduced into the vagina.

Dr. Tilt has recommended the use of cotton wool, with which the medicines to be used are to be incorporated, and the cotton placed in the vagina by means of the speculum, a thread being attached to the cotton by which the patient may remove it. In some cases I have used this method, but a pessary that could be introduced by the patient herself is in most cases more useful.

The great difficulty is to get the pessaries so made that they will be firm enough to be introduced by the patient herself, and yet melt at the temperature of the body. In the fourth volume of the *Transactions of the London Obstetrical Society* there is a paper by Dr. Tanner, recommending the use of the butter of the cacao theobroma for this purpose. This substance was brought into notice by Mr. White Cooper as a foundation for ophthalmic ointments, in a paper published in the *Lancet* in June, 1862, and Dr. Tanner recommended it for pessaries because of its being firm at low temperatures, but melting at that of the body. Since the publication of Dr. Tanner’s paper, I have made repeated efforts to obtain pessaries made as he recommends, but without succeeding in getting them made of a uniform character—as they would sometimes be too soft, at others too hard.

In the *Edinburgh Medical Journal* for May last there is a paper by Sir

James Simpson describing pessaries and suppositories he is at present in the habit of using; and during a visit to Edinburgh, in September last, I obtained some of those from Messrs. Duncan and Flockhart, which on my return I showed to some of our Dublin apothecaries, and asked them to make me similar ones. Mr. W. Pakenham, the present proprietor of the establishment in Henry-street, instituted a series of experiments for the purpose, and has succeeded in making both pessaries and suppositories such as I now exhibit alongside of the Edinburgh ones, to which I believe they are fully equal, they will, I think, be found to fulfil every purpose.

There are three substances in the market derived from the fruit of trees of the order of *Palmæ*, and each of them called cocoa-nut butter.

1. Palm oil, derived from the fruit of the *cocos butyracea*. 2. *Coco olein*, derived from the kernel of the cocoa nut—the fruit of the *cocos nucifera*. 3. The produce of the *cocos theobroma*, the substance recommended by Mr. White Cooper for ophthalmic ointments, and adopted by Dr. Tanner, for pessaries, which is obtained from the fruit of the chocolate tree.

Of these substances the palm oil may be dismissed as unfit, from its softness, for the purpose required. The butter of the *cocos theobroma*, of which I show a specimen, obtained from the London Exhibition of 1862, is a firm, hard substance, which, if it were easily obtained and were certain to melt at the temperature of the body, might answer very well for forming pessaries, but it is not common in the market, and seems not to melt very easily. On applying at different establishments for specimens of it, and even at the same establishment on different occasions I have never twice got the same substance. The specimen I show is part of one obtained by Mr. Tichborne, of the Apothecaries' Hall, from the Exhibition of 1862, and by him given to Mr. Pakenham.

The *coco-oleine*, or butter from the cocoa-nut, the fruit of the *cocos nucifera*, is easily procured. It has the pleasant odour of the cocoa-nut. At the ordinary temperature of the air it retains a solid form, but it melts too easily to be used by itself for pessaries. Mr. Pakenham has found that by mixing it with certain proportions of wax it acquires firmness, and yet retains its power of melting at low temperatures, and it is a combination of this kind that he uses.

Almost any medicinal substances may be made into pessaries, or suppositories, with this combination, and by making them in moulds they are given a conical form, which greatly facilitates their introduction either into the vagina or rectum, where they melt very rapidly.

Patients have sometimes objected to the use of medicated pessaries, because they melt so rapidly, and, running out of the vagina, soil their clothes. To obviate this, I have been in the habit latterly of having them made as small as suppositories, but there is a difficulty in introducing these, and some substances that are often very useful are too

bulky to be so used, such as the oxides of bismuth and zinc, tannin, iodide or bromide of potassium, &c.; and I have been trying some other substances as a foundation, such as starch boiled in glycerine, but without success for so far.—13th January, 1866.

DR. DENHAM related a case he had recently been called to, in which, though the patient was in labour of her first child, the hymen was unruptured, and presented only a very small opening anteriorly.—13th January, 1866.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.^a

SESSION 1864-65.

DR. DAVID CREMEN, President.

Notes on Delirium Tremens. By R. W. DAY, M.D.

CASE I.—I was called to see Captain H., of an American vessel, which had just arrived from Liverpool late this evening. I found him walking up and down his cabin in a most excited state. He told me he had no rest of any kind for four nights; that since leaving Liverpool not a particle of any food had passed his lips, but that he was perpetually tipping *gin*, having consumed two gallons of it!! (This was subsequently verified by his mate.) He complains of intense weakness of his limbs, a burning heat along the back of wrists and hands, and great fulness in the head and behind the ears. The tongue very much coated; skin very slightly moist; bowels constipated; face flushed; eyes injected and ferrety. Ordered, hyd. submur., gr. 6.; jalapæ, $\frac{1}{2}$ drachm; olei. crotonis, gtts. iv. M. Mellis, q.s., for bolus. Statim. And when the bowels were relieved to have 1 gr. of morph. murias.

Nov. 1st.—Bowels affected four times during the night; stomach very irritable; vomiting enormous quantities of bile; the pill was also rejected. Ordered a saline effervescent mixture and four pills, each to contain 1 gr. of opium (crude), one every second hour. The uneasiness about his head is much relieved; skin more perspirable; tongue cleaner; pulse soft and full; the muscular tremor greatly increased, as all stimulants were withheld.

Evening.—He has taken the four pills without enjoying any sleep; the irritability of the stomach has ceased; the bowels twice well affected since morning; the muscular jerkings very annoying. Ordered tinct. opii m. xl.

^a These reports are supplied by Dr. Curtis, Secretary to the Society.

2nd.—In an hour after taking the draught he fell off into a deep sleep, which lasted six hours. I found him discussing a hearty breakfast of chops and wildfowl. The tremors have gone; his tongue is perfectly clean; no headache, but a slight feeling of lightness; says he feels quite able to go out and attend to his duties.

In this case my reason for cutting off the supply of stimulants was that I considered the nervous exhaustion present was not intense enough to demand them, and I am fully convinced that their exhibition in even small quantities would have retarded the convalescence I was so desirous of bringing about. Since this date, I have attended four-and-thirty bad cases of delirium tremens successfully, adopting, with slight modifications, the same line of treatment. The last case that came under my notice, in September of this year, was characterized by the most violent spasmodic jerkings I had ever seen. It was the third attack in two months the gentleman was suffering from. I gave chloroform in half drachm doses with magical effect; two draughts of it sufficed; the spasms vanished; sickness of the stomach, which was annoying considerably, was allayed, and sleep induced.

I append to these imperfect notes the history of a case kindly sent to me by my friend Staff Assistant-Surgeon Wright, which may prove worthy of the notice of the society.—October 31, 1861.

Notes of a Case of Delirium Tremens, from Staff Assistant-Surgeon T. WRIGHT, M.D., Cape St. Mary, coast of Africa.—The patient was a sergeant-major, and had been drinking very heavily for days past. On first entering hospital the treatment commenced with morph. acet. in grain doses, but without effect. Battley's solution was then ordered in half-drachm doses every three hours. Of this preparation he got twelve doses without inducing sleep, or even quieting him. Although he was most violent, and required the care of four men incessantly to keep him in the ward. A consultation was now held, and it was determined to try the digitalis, as first recommended, I believe, by Mr. Jones, of Jersey. Accordingly half-an-ounce of the tincture was given, and ordered to be repeated every six hours. He had three doses; the last told. The pulse, which was 120, came down to 110, and he fell into a profound sleep, which lasted fourteen hours, awaking cured.—Sept. 23, 1865.

Two Cases of Paraplegia, with Remarks. By E. R. TOWNSEND, JUN., M.D., Physician to the Cork South Infirmary and County Hospital:—

A Case of Acute Paraplegia depending on Meningitis of the Spinal Cord, under the care of E. R. Townsend, Jun., M.D., T.C.D.*—Jehan Vidozie, aged forty-one, an Austrian sailor, was admitted into the South Infirmary

* Reported by Mr. J. J. Fitzpatrick, clinical clerk.

and County Cork General Hospital, May 13th, 1865, labouring under complete paraplegia. History of the case:—Had been at sea during very severe weather, and was constantly wet through, and one night he slept in his wet clothes. On the Monday before admission he suddenly lost the use of his legs and fell on the deck. When he got up he was barely able to stand, but could do so with support. He was immediately conveyed to his berth, and very soon lost all power over his lower limbs; they also became quite rigid, so that he was unable to stir them at all. Symptoms on admission:—Complete loss of motion of both lower extremities, from the waist downwards, with greatly increased sensibility; limbs quite rigid; recti-abdomini muscles also quite rigid and tense. Screams and grinds his teeth if he is touched on the legs or abdomen; cannot even bear the pressure of the bed-clothes. The spasms brought on by even slight pressure with the fingers over the recti muscles almost amount to those of tetanus. His countenance is expressive of great anxiety and suffering, the corrugatores supercillii being spasmodically contracted. The surface of his body is bathed in a profuse perspiration; pulse calm, 80; has perfect control over both rectum and bladder; urine acid. suffers much from priapism; has a painful sensation, as if a cord was tied round his waist.

Treatment, May 14th.—Ordered gr. 5. of iodide of potassium every fourth hour.

15th.—Complains of intense pain over the sacrum and lumbar vertebræ; cannot bear to be touched; grinds his teeth and screams when even gentle pressure is made over the spine, particularly over the lumbar region. Sixteen leeches to be applied over the upper part of the sacrum; cupping glasses to be applied over the bites. Ordered \mathcal{R} secale cornuti, gr. 3, in pulv. ter in die; to continue mixt. potass. iod.

16th.—Pain in back still continues; has not slept since his admission. Ordered twelve leeches over the lumbar vertebræ, to be followed by the cupping glasses; to take three grains of ergot in powder every six hours; to continue the iodide of potassium every three hours; to take beef-tea; his appetite is good, and he eats bread with a liking; is very thirsty.

17th.—Twelve leeches to be applied over the spine, where the pain is most intense; to be followed by the cupping glasses; nates to be sponged with whiskey; pulse 80; does not appear to be suffering so much pain; stirs the toes of one foot slightly, but cannot bear to have his legs touched. No reflex movement produced by tickling the soles of the feet, but it gave him so much pain the experiment could not be repeated. Ordered, tinct. hyosciami \mathfrak{z} i., aquæ \mathfrak{z} i.; fiat haustus hora somni sumendus; this to be repeated in three hours, if he does not sleep.

18th.—Refused to take the draughts last night; did not sleep; is constantly groaning and screaming at night with pain. Since the last application of the leeches he has ceased to feel the cord-like constriction

round his waist. To continue the ergot and iodide of potassium. Ordered, tinct. hyosciami, $1\frac{1}{2}$ drachms; aquæ, 1 oz.; fiat haustus; to be taken at night, and repeated in four hours, if he does not sleep.

19th.—Took the draughts, but did not sleep; says the pain kept him awake. Ordered twelve leeches over the dorsal region of the spine, to be followed by the cupping glasses. R. Extracti hyosciami, gr. 3; fiat pil. 1; h.s.s.

20th.—No sleep; very restless, and hard to get him to take his medicine. Ordered, R. Pulv. secale cornuti, gr. 3; extracti hyosciami, gr. 1; fiat pil. 1; mitte tales, 12; sumat, 1 quartis horis; applicentur cucurbitulæ sine ferro dorso. To continue mixt. potass. iod.; beef-tea, &c.

21st.—Had some sleep last night, and has become quieter and more manageable; the peculiar anxious expression of countenance is less; the entire trunk and extremities are covered with sudamina, resembling the eruption caused by croton oil.

22nd.—Improved. To be dry cupped along the spine. To continue his pills and mixture.

25th.—Rigidity of lower extremities less; sudamina disappearing, though perspirations still continue; the peculiar anxious and contracted state of the corrugatores supercilii, and of the other muscles of expression of the face has subsided altogether; countenance is now natural.

26th.—Complains of some slight increase of pain in the lower extremities; to be cupped over the dorsal region of the spine. Several ounces of blood were taken by this means, after which he felt much easier.

29th.—To be dry cupped along the spine; is now able to flex the knees a little, and can turn himself on his side by means of a cord suspended from the ceiling; is much better in every respect.

30th.—Pain in limbs greatly decreased; can move the limbs better; can raise the legs up a little, and can flex the knees fully. To be dry cupped again along the spine. To continue the mixture of potassii iodidi and the pills.

31st.—A belladonna plaster, six inches long by two wide, to be applied to the spine; the urine for the first time proved to be alkaline.

June 1st.—Was taken out of bed and put sitting in a chair, to which he was fastened for support; complained of intense pain in his legs, with great rigidity of gastrocnemii muscles, but was glad to change from the bed; sat up several hours.

4th.—Has sat up every day since, and says he feels much better. To continue everything.

6th.—Contraction and rigidity of the muscles much less; is able to move both legs well, and is even able to stand on them with support, but says it gives him intense pain in the calves of his legs. He stands on

the toes like an opera dancer, and cannot place the soles flat on the ground. He cannot bring the heels to the ground at all.

7th.—Was taken out into the garden to-day, where he remained the entire day. He was allowed to smoke for the first time, and seemed to be quite free from pain, but the muscles are still very rigid.

8th.—Is now able to get out of bed without assistance; the gastrocnemius muscle of right leg is much more contracted and rigid than that of the left. Only complains of slight pain on touching the limbs; can now walk round the table in the ward, holding on by it. To have two ounces of wine at dinner.

11th.—Since last report has been improving rapidly, every day walking better. He can now walk without any assistance; the toes are still slightly drawn along the ground; still complains of a little pain and stiffness from the knees downwards. The urine has been constantly tested, and always found to be acid, except on the one occasion mentioned.

12th.—To omit the ergot pills and the potash mixture; can walk up and down stairs quite well.

14th.—To rub the calves of the legs with the stimulating liniment, as he still has some stiffness in them; complains of perspiration at night. Ordered to take one ounce of the following mixture three times in the day:—*R.* Quininae sulph., gr. 8; acid sulph. aromat., ʒiss.; aquæ, ad ʒ viii.; fiat mistura. This he continued to take until the 21st, when he felt quite well. He was able to walk as fast as ever he did before his illness, but could not run well, in consequence of the stiffness remaining in the gastrocnemii muscles. He was able to climb up a tree.

He left the hospital on the 23rd June, with the power of motion fully restored in both legs, and the sensation perfectly normal. He felt quite able to join his ship, and said he thought he could go aloft as well as ever.

This man was admitted into hospital on the sixth day from that on which he first lost the use of his limbs; the paralysis came on suddenly, and was obviously caused by his sleeping in wet clothes after the hardships he had undergone during very severe weather at sea. He had previously been a very healthy man. The suddenness of the seizure is remarkable in this case; he was standing on the deck when he suddenly lost the use of his legs, and fell down; on being taken up could not stand without support. He was immediately conveyed to his berth, and, soon after being placed in bed, completely lost all power over his lower limbs, and they became quite rigid. This aggravation of his symptoms on being put into bed was manifestly caused by the tendency which the horizontal posture has of increasing the congestion of the spinal cord, and should be borne in mind in treating cases of paraplegia due to that lesion. From the history and symptoms of the case I diagnosed it to be

inflammation of the membranes of the cord, with exudation sufficient to cause pressure on the cord, or on the nerves leaving the cord, complicated with congestion of the cord itself, and probably some effusion into the lower part of the vertebral canal. That the meninges of the cord were in a state of inflammation was evident from the rigidity of the muscles, both of the back, abdomen, and lower extremities—from the intense pain caused by any pressure on the back—from the violent spasms, which were almost tetanic, and from the pain which the slightest pressure, or even the slightest touch, of the abdominal muscles, or the least motion of the lower extremities, gave rise to—in fact, from the extreme state of hyperesthesia of the lower extremities, and also from the acidity of the urine.

Ollivier, in his work on the spinal cord, Vol. II., Chap. VII., says there are two symptoms which may be considered as pathognomonic signs of acute inflammation of the membranes of the cord, since they are, if not always, at least frequently, combined; the first consists in a general contraction of the muscles of the posterior part of the trunk, which may vary from simple muscular rigidity to the most violent contraction. This tetanic contraction manifests itself principally when we wish to make the patient move; and it even happens that it does not exist when the body is in a state of complete rest. The muscular rigidity is seated principally in the trunk, without the extremities participating in it. These, however, are often equally affected. The second symptom is pain, more or less acute, in the dorsal region; it seems to commence, in general, from the point where the inflammation is most intense, and there, also, it is always most acute. It, as well as the muscular rigidity, presents remissions, and sometimes even disappears in order to manifest itself anew. The pain may exist without any perceptible muscular contraction, but it is ordinarily accompanied by this phenomenon or by convulsive motions, and uniformly the rigidity of the muscles manifests itself immediately or a little time after the pain has appeared. The violent pain complained of by patients, along the spine, is a constant symptom of spinal meningitis; sometimes it darts rapidly from the point primarily painful to the entire extent of the back; its radiations are extended to the limbs, and the slightest pressure, or a mere displacement, makes the patient scream aloud.

This morbid exaltation of the general sensibility is an almost constant phenomenon in spinal meningitis, and there is commonly observed no diminution of this property, a circumstance which assists in distinguishing this inflammation from myelitis, which is ordinarily accompanied with a more or less complete abolition of the sensibility; thus this sign may serve to characterize inflammation limited to the membranes of the spinal cord, since in the one the sensibility is exalted, and in the other it is weakened or destroyed. The combination of these two symptoms

characterize, in a manner, acute inflammation of the spinal meninges, for in all the cases in which both were observed the *autopsy* showed an inflammation of the entire extent, or of the greater part, of these membranes; and but rarely the inflammation occupied but a very limited space. To these symptoms must be added, more or less acute pains in the limbs, with greater or less rigidity of these same parts. Ordinarily copious sweat covers the patient during the attack when the tetanic contractions are intermittent. In this case, from the greater intensity of the pain over the lumbar region, it was evident that the disease was principally situated in the lower part of the cord, more particularly in the lumbar part of it, though for several days there was a manifest tendency to spread up along the dorsal region. The seat of the inflammation in cases of spinal meningitis, and also in myelitis, must influence the prognosis very materially; the higher up the seat of the lesion the greater the danger to the life of the patient. Should the lesion be seated in the dorsal region, between the two enlargements of the cord, the respiratory muscles, which are under the influence of the dorsal spinal nerves, are frequently agitated by violent spasms, and the breathing is accomplished by short and painful efforts; if the disease extends higher up deglutition becomes difficult, the respiration abdominal, and the patient soon sinks from effusion into the bronchial tubes. It has been generally noticed that the symptoms of irritation and inflammation of the spinal cord are much more early manifested, and are generally more serious in their consequences, when the dorsal region is affected than when either the cervical or lumbar are the seat of the disease. This has been accounted for, by Mr. Earle, from the construction of the spine. In the dorsal portion of the spine the calibre of the canal is narrower and more closely adapted to the volume of its contents than in any other part. In the cervical vertebræ, where the extent of motion between vertebra and vertebra is greater, the canal is of a triangular form, and large in proportion to the size of the cord; in the lumbar vertebræ it is also triangular and capacious, and the dura mater is loosely connected with the bony canal; a considerable space, moreover, is left between the opposed surfaces of the arachnoid, so as to allow of a sufficient play of one surface on the other, and thus at the greatest extent of natural curve no pressure can take place; hence in the dorsal region the slightest congestion or effusion is productive of more serious symptoms (from the canal being smaller and more completely filled with the cord and its membranes) than in the lumbar region, where, from the greater capacity of the canal and looseness of the membranes, even considerable effusion may exist without producing such serious consequences; and, therefore, the prognosis is more favourable when the inflammation is confined to the lower part of the cord or its membranes than when it extends or commences higher up. The treatment adopted in this case was, at first, the

application of leeches and cupping-glasses where the pain in the back was most severe, and after each application he felt relieved; after the third leeching he lost the cord-like sensation round his waist. When the severity of the pain was somewhat diminished dry cupping was tried with very good effects; and when, after some days, the severe pain returned, it again yielded to the application of the cupping-glasses, with the scarificator. The internal treatment was, from the first, by the administration of iodide of potassium in five-grain doses, every four hours, afterwards increased to every three hours; he was also put on ergot of rye, at first in three-grain doses, every six hours, and, as it produced no unpleasant effects, after three days I directed it to be taken every four hours, combined with extract of henbane; these pills he continued to take from the 16th of May to the 12th of June, thus taking eighteen grains of powdered ergot daily, for nearly a month, without any unpleasant consequences, and with the very best effect, the result being most satisfactory. This is now the fourth case of paraplegia in which I have used the ergot of rye, and in each case the effects have exceeded my most sanguine expectations. This drug was first suggested, in cases of paralysis, by M. Barbier, of Amiens, who considered it as a remedy possessing the same power as strychnine, but in a more manageable form; and M. Payan, of Aix, reasoning on the special action which the *secale cornutum* seems to have on the nervous system, producing, as it does, feelings of tingling and involuntary spasmodic movements of the legs, tried its effects in paraplegia, and in many cases with the most encouraging results.

From repeated and careful observations he concluded that where neither great pressure (not beyond that which simple congestion produces, nor disorganization of the spinal cord exists, its remedial power is very great. It is especially of use in cases where there is paralysis of both rectum and bladder. At the Bicêtre, M. Guersant has established the fact of its efficacy as a means of resuscitating muscular contractility of the rectum and bladder, and of assisting the latter organ to expel the remains of a calculus left after lithotrity. Brown-Séquard says that of all the medicines employed internally in cases of paraplegia, the two that are most powerful in diminishing congestion of the spinal cord are belladonna and ergot of rye, and that he has seen the diminution in the calibre of blood-vessels of the pia mater of the spinal cord taking place in dogs after they had taken large doses of belladonna or ergot of rye. He has also ascertained that the reflex power of the spinal cord becomes very much diminished under the influence of these two remedies, which in so doing act just in the opposite way to that of strychnine; he therefore advocates the use of ergot of rye in cases of paraplegia due to congestion or inflammation of the spinal cord or its membranes. As soon as this man was able to bear any movement he was placed as much as possible on his side, and changed from side to side constantly. After a little time

he was able to turn himself on his side, by means of a rope suspended from the ceiling; two days after this I had him taken out of bed and placed in an arm-chair, to which he was obliged to be fastened to keep him from falling forward. From this day he rapidly improved; indeed, from the result in this case, and also in M'Carthy's (see *Dub. Med. Press*, July, 1864), I look upon change of posture, from the horizontal to the erect position, or as near to it as possible, of the utmost importance in the treatment of all cases of paraplegia, either caused by or complicated with congestion of the cord or its membranes.

For the next case I am indebted to Dr. W. C. Townsend. The patient, Alexander Scott, aged thirty, was admitted into the South Infirmary, under the care of Dr. W. C. Townsend, May 31st, 1865. History:—Was a labourer at Beamish and Crawford's brewery, and for five weeks had a dull pain over the lumbar region, which he ascribes to his having, whilst intoxicated, slept out one night in wet clothes. The pain continued until the 29th of May, two days before his admission, when he first noticed, on getting out of bed, that he had lost the use of his right leg. He was unable to stand or lean any weight on it. He applied to the dispensary doctor, who blistered him very severely over the lower part of the spine. Not feeling any better he applied at the hospital, and was admitted on the 31st of May. Symptoms on admission:—Complete loss of motion in the right leg, from the waist down. Sensibility somewhat increased in the affected limb. The temperature of the right leg is higher than that of the other; appetite bad; perspires at night; complains very much of the soreness of his back, from the blister.

Treatment, May 31st.—Blue pill and squills at night, and sulphate of magnesia in the morning.

June 1st.—Repeat pills at night; back to be dressed with simple cerate; symptoms same as on admission.

3rd.—To continue the pills,

5th.—Ordered two grains of quinine in two ounces of sherry twice a day.

6th.—Temperature and sensibility of both legs nearly equal; blister on back nearly healed.

8th.—Is able to stir the toes of the affected leg very slightly. Continue pills and quinine.

9th.—Ordered the mixt. of potassii iodidi, gr. 5, ad aquæ, oz. 2, ter in die; to omit the quinine, but to continue the wine, four ounces in the day; symptoms same as last report.

10th.—To have a warm bath.

11th.—To continue mixt. potass. iod. Electricity to be applied to the limb.

12th.—Symptoms much the same. He continued in this state, and

under this treatment, up to the 21st, when, during the absence of Dr. W. C. Townsend, having come under my charge, I determined to try the ergot of rye with him, still continuing the iodide of potassium.

On June 21st I ordered him the following:—*R.* Pulv. secale cornuti, gr. 3; pil rhei comp., gr. 2; extracti hyosciami, gr. 1; fiat pil, 1; mitte, 12; sumat, 1; ter in die.

23rd.—Power over right foot appears gradually increasing; makes an effort to stir the toes slightly.

25th.—Stirs the foot better, and can flex the knee a little; has a feeling of numbness over the entire limb.

27th.—Same as last report.

28th.—Complains of a nervous feeling over him; has more power of motion in the leg. To omit the ergot and take pil galb. cum. col., 2; statim; to be followed by a draught of infusion of senna with valerian and sal volatile. To continue the iodide of potassium mixture with the addition of aromatic spirit of ammonia.

29th.—Resumed ergot pills.

July 1st.—To be dry cupped along the spine; to take only one pill at night.

2nd.—Feels better after the dry cupping; is able to walk a little with assistance, but drags the leg along the ground; the muscles of the limb are somewhat wasted.

4th.—To be dry cupped again along the spine; to continue the pill and mixture.

6th.—Same way.

9th.—To take the iodide of potassium in infusion of calumba, as his appetite was not as good as it was.

11th.—Complains of a tingling sensation in the right leg, which commences in the toes, and gradually extends upwards along the limb.

14th.—Omit ergot pills. Continue mixt. potass. iod.; feels better.

16th.—Motion and sensation much improved in right leg; temperature rather lower than in left leg.

17th.—Omit mixt. potass. iod.; to take quinine in sherry twice a day.

21st.—He left the hospital very much improved, able to walk tolerably well with the assistance of a stick; sensation quite restored, and the temperature the same in both legs. Since then he has attended as an extern patient, and is now able to walk as well as ever. The last time I saw him was about a month since, when he walked up from Riverstown, a distance of six miles, without the slightest difficulty. He said his right leg was now almost as strong as the left.

This case shows that ergot has a very powerful effect on the spinal cord. This man was taking iodide of potassium for some time before he came under my care, and had improved up to a certain extent, but did not seem to be progressing beyond this point. After a week or

ten days, seeing no change in him, I determined to try the ergot, still continuing the iodide of potassium, and in three days from his first taking the ergot a marked improvement took place in him, and from this out he continued to improve, and left the hospital very much better. The ergot in this case caused some unpleasant effects, which obliged him to omit it occasionally, and he was never able to take more than three grains, three times in the day, without its producing tingling in the affected leg; it also disagreed with his stomach, so that the dose had often to be reduced to three grains once in the day.

On the Constituents of Food, and their Relation to Muscular Work and Animal Heat. By F. C. DONDERS, M.D., Professor of Physiology and Ophthalmology in the University of Utrecht. Translated from the *Nederlandsch Archief voor Genees en Natuurkunde*, 1^e Deel, Utrecht, 1864, by WILLIAM DANIEL MOORE, M.D., Dub., M.R.I.A.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in *Materia Medica* and Medical Jurisprudence in the Queen's University in Ireland.

I. INTRODUCTION.

THE rational theory of nutrition commences with the discoveries of Mulder in the year 1838.

In the establishment of the fact that the albuminous matters of plants and of animals agree in composition a foundation was obtained on which observers might safely build. Each subsequent investigation started from this proposition: plants prepare the albuminous matters of animals—and with this were closely connected the numerous questions respecting the theory of nutrition, and the metamorphosis of matter, with which thenceforward physiologists occupied themselves. It was tacitly assumed that the animal organism can form no albuminous matters, of which formation the grounds are even still wanting. In the supply of albuminous matters, therefore, a condition of animal life was found.

But it was known, and this fact received further confirmation, that vegetable food is relatively poor, while animal food is rich in albuminous matter. And, inasmuch as whole classes of animals live exclusively either on the one or on the other, it was evident that animal life may be maintained with little albumen, but that it may also use much albumen.

What was true in this respect of man?

The structure of his body, and not less his inclinations, wherein the conditions under which our race is laid are manifested, indicate

for him a place between herbivorous and carnivorous animals. Both kinds of food, it was inferred, were destined for his use. Two circumstances, however, could not escape attention. In the first place, that whole classes of our fellows are doomed to be vegetarians; in the second place, that in these classes we find least of all examples of the bodily and mental perfection of which we think our race is capable. Comparative considerations appeared further to justify us in seeking the cause of this inferior development, in part at least, in an imperfect supply of albuminous matters. The desirability, nay the necessity, of a more liberal supply was now strongly insisted on. Particularly the want of strength, or rather the slight power of work or energy, was referred to the deficiency alluded to. The fact was adduced that the muscles which perform the work consist for the most part of protein matters, and the metamorphosis of these muscles was looked upon as a necessary condition of their energy. Liebig—but Liebig alone—went, in his zeal for striking antitheses, even so far that he deduced all mechanical power directly from the use of organised albuminous matters, leaving to the fats and so-called carbo-hydrates only the inferior part of serving as heat producers. To such perilous propositions physiologists have, however, not committed themselves. It is indeed true, that we find in Bischoff and Voit, whose investigations were from the first animated by Liebig's spirit, still a feeble echo of the same doctrine; but we must be strangers in the domain of physiology to dispute the point with them, as if it had obtained general acceptance.

Of late years the principle of the maintenance of energy, so fruitful in its results, has been more and more applied to the phenomena of living nature. Moreover, new and important facts have been brought to light, which exercise a very essential influence upon our views respecting the relation of muscular work and of heat to the metamorphosis of matter. I refer especially to two, namely, the cooling of the blood in the lungs, and the slight increase of the urea in the urine, after the performance of muscular work. The first is important in its bearing on the theory of respiration, and on the place where the heat is produced. The second led to the conclusion that in muscular work the tissue is not wasted, but only gives the impulse to the metamorphosis of non-nitrogenous bodies, by transferring oxygen to them (M. Traube). One of our fellow-countrymen ventured still further, publishing a pamphlet, which leaves the impression on the reader that the use of animal food is to be considered, if not injurious, at least as rather superfluous for man.

It appears to me that such a statement should not be left unanswered. It is true that in general it is scarcely necessary to combat an error in the domain of science. Left to itself, it will almost always soon be forgotten, and so dies a natural death, which is also the strongest security against its resurrection. In practical life it is otherwise. Here tradition

keeps itself, through incompetent, still more than through competent judges, alive from mouth to mouth. Here it may therefore be expedient, if possible, to stifle in their birth incorrect ideas where they threaten to creep in. Let, however, no controversy be expected from me; I wish, without entering at large into the above doctrine, simply to record my own views upon the subject. Independently of what has been written by Traube and by Beins, it was certainly not unimportant to consider what idea respecting the origin of muscular work and heat the present state of science justifies. To this the following pages will be devoted.

II. BALANCE OF ENERGY IN THE ANIMAL BODY.

Never was a law received with such general approbation as that of the "indestructibility of force." When it was formulized, it appeared as if men had already been acquainted with it for years—had lived, thought, and worked under its influence. It forced itself upon us as a logical necessity. Poor in decisive proofs, it stepped into life; but we had intuitively the conviction that no facts would refute it, and it was used, forthwith, almost rather as a test of the correctness of observations than that it was thought necessary to check it by fresh investigations.

In the first contributions of J. R. Mayer,^a in 1842, the principle is already fully expressed. Mayer adopts forces in the sense of *causes*, and starts from the fundamental proposition—*causa "æquat" effectum*. Therefore, he says, the effect is in its turn a cause, and herein lies its "indestructibility." But its form may alter, and it is thus "changeable." Mayer's treatise comprised the indication of the chemical energy, which in the combination of bodies is manifested as heat, a correct description of the relation between heat, motion, and gravity (a lifted load); lastly the idea of mechanical warmth-equivalent, which he also already expressed in "gravity"—in "kilogrammeters."^b

^a *Annalen der Chemie und Pharmacie*. Bd. xxxii.

^b A kilogrammeter is the *work* necessary to bring a kilogramme (2.2055 lbs. avoirdupois) to the height of a mètre (3.2808992 English feet). By this work we have obtained a kilogrammeter "gravity" as energy. While the word "force" had in mechanics a definite signification, according to which it might be stated as a pressing weight, in kilogrammes, the words *work* and *energy* have been adopted in the first mentioned sense, instead of the word "force" still used by Mayer. A second measure, whereby to express it, is that of calories: a calorie is the amount of heat necessary to raise the temperature of a kilogramme of water by 1° C. (1.8° F.). Now the mechanical equivalent of heat is expressed by the number of kilogrammeters, which are the equivalent of a calorie, that is about 430; that is to say, that a calorie is used to raise one kilogramme to the height of 430 metres, and that, *vice versa*, a kilogramme, falling from the height of 430 metres, can produce one calorie. We easily obtain a just idea of work and energy from the measure with which they are measured. Any one who desires more exact ideas respecting this fundamental matter, may be referred to Helmholtz (*Ueber die Wechselwirkung der Naturkräfte: on the Reciprocal Action*

Mayer's essay passed unobserved by many; our highly esteemed colleague, v. Rees, immediately perceived its importance, and directed the attention of his friends to it. It was now easy to apply it to physiology. At the close of a public lecture,^a delivered in the Winter of 1844, I remarked, that it was not sufficient to deduce only the heat developed from the metamorphosis of matter, but that in addition some other force (say work) must have its cause therein; and that only when neither electricity, nor light, developed by some animals, nor mechanical power, which proceeds from all, was lost externally; when therefore all were changed wholly into heat, could the quantity of heat developed correspond to the oxidation of the organic matters; finally, that these matters had been developed in plants under the use of the sunlight, so that we properly find, in the vital phenomena, the equivalent of the light used. All this, and much more, we find accurately and clearly set forth in the pamphlet soon after published by Mayer:—*Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel*. Heilbron, 1845.

Even earlier we find the principle of the maintenance of energy, without being as yet expressed, here and there correctly applied. When, for example, the efficacy of nerves and muscles as a source of heat was assumed, it was generally supposed that this efficacy had required as much chemical action as would have been necessary for the immediate production of the same heat. Liebig had even stated, that the heat which our muscular action can excite outside the body by friction, is as much a product of the metamorphosis of matter as the heat developed in the body itself. But no one was found to be faithful to the principle, and many a proof of inconsequence might be adduced.

Even in 1822 Dulong, and shortly afterwards Despretz, had compared the heat developed in animals with the oxidation in the body, and they had arrived at the result that at least the greater part, 0·7 to 0·9, of the heat given off can be deduced therefrom. In the calculation it was supposed that in organic food the oxygen is combined with hydrogen, and that the remaining hydrogen and carbon with the oxygen taking part in the combustion still develop as much heat as if they had been present in the elementary state. Thus for the burnt carbo-hydrates only so much

of Natural Forces. Königsberg, 1854), and to Bosscha (*Het behoud van arbeidsvermogen in den galvanischen stroom: on the Maintenance of Energy in the Galvanic Current*. Leiden, 1858),—in which two writings the profundity of the subject has not suffered from the popular mode of treating of it. We may here further remark, that we have to distinguish between energy at rest, *potential energy*, Rankine (a raised load which can fall), and energy in motion, *actual energy* (a body in motion, *force vive*). What both can perform is *work*, of which the result may in its turn occur as energy.

^a *Blik op de stofwisseling als bron der eigene warmte van planten en dieren*. (Glance at the metamorphosis of matter as the source of the proper heat of plants and animals). 8vo, Utrecht. January, 1845.

heat was brought into calculation as the carbon therein existing, in the free state, would have developed. Helmholtz^a proved the incorrectness of this statement. The heat arising from the combustion of alcohol had been known from the investigations of Favre and Silbermann, and a simple calculation showed that the alcohol alone, although formed with development of heat from sugar, in combustion produces more heat than the carbon of all the sugar which had served for the formation of alcohol would be able to do in the *elementary state*. By this consideration the deficit of Dulong and of Despretz was explained, without any necessity for assuming, with Liebig and with Mayer, a greater production of heat in the combustion of carbon in the elementary condition. It should be observed, that in the experiments of Dulong and of Despretz, the animals were at rest, and, so far as they might have moved, this movement here also was converted into the measured heat. According to the principle of the maintenance of work, only this, therefore, needed to be capable of being deduced from the chemical metamorphosis.

A few years before Mayer had expressed the great principle, physiologists had occupied themselves much with the balance between the matters conveyed into and out of the body. If neither in this manner did they penetrate into the proper laboratory of life, they obtained a starting point for further investigation: from the knowledge of the composition and of the quantity of the excreted matters, they ascended to the question, where and from what compounds were they formed, and by this they were led to analyses of the blood and of the tissues, which gave the answer to many a question.

In reference to the energy, the same task had now to be fulfilled. Here also it was necessary to begin by making up the balance. A certain quantity of chemical energy was introduced, under the form of organic food and oxygen, and the equivalent of what of this was used, is found again in the heat and in the mechanical work proceeding from the body. The two balances, that of the matter and that of the energy, form a whole. The thing is simple. The ingested and the egested elements are the same. Their quantity too has remained unaltered. Only the forms of combination differ, and with them the energy. In the combustion of what is taken in we obtain, in fact, much more heat, than in the combustion of what is excreted: it is the difference in their energy. Now to this difference corresponds exactly the sum of the heat developed in the body and of the heat-equivalent of the mechanical work yielded externally by the body.

If we know the value of the three factors mentioned the balance is ascertained. Other forms of energy do not enter into it. The light,

^a Encyclopädisches Wörterbuch der medicinischen Wissenschaften. B. xxxv. Berlin, 1846. Art. Wärme.

which presses into the eye, the vibrations communicated to the auditory nerve, may physiologically be of great importance, quantitatively they are of none in our balance. To electric currents also, coming from without, the body is not, under ordinary circumstances, exposed, and the heat, which we, directly or indirectly, receive from without, remains heat, and passes as such (free or combined) again away from us. Thus nothing else is introduced than the chemical energy of our food and of the oxygen. And, as to the outgoing energy, we have, except what remains in the excreted matters, to do exclusively with heat and with mechanical work. Indeed, no perceptible loss of electricity takes place in man, and the so-called animal magnetism is a suggestion of ideas, no natural force.

But this balance having been ascertained, our task is not yet complete; it is only marked out. In the body numerous metamorphoses take place, which we must endeavour to follow step by step. But it would be quite complete had we succeeded in tracing the origin of the heat and of the mechanical work from the chemical energy, mediately and immediately. A wide field lies here before us, in which the seed begins here and there to shoot up, but the harvest is still remote.

III. DIRECT AND INDIRECT PRODUCTION OF HEAT.

Heat is in general the final product of change of energy. What occurs in nature as heat can only in part return to another form, and what cannot be communicated to a colder body remains heat for ever. Thus it is too in the animal organism. Just as water, carbonic acid, and urea are final products of the metamorphosis of matter, heat is the last form which the energy assumes. The heat which has once arisen in the body must remain heat, because it has no colder parts to warm. It is the excretum of the energy, partly disappearing as free heat externally, partly existing combined in the excreted gases and in vapour.

Heat arises in the body directly or indirectly, that is either as the immediate result of chemical action, with the sacrifice of chemical energy, or from other forms of energy, which equally were the result of chemical action.

Formerly the direct production of heat was assigned a prominent place. It was admitted that in the lungs, by chemical combination of the oxygen with constituents of the blood, much heat was developed, and it was supposed that, in spite of cooling through warming the air and the development of gases and watery vapour, the blood here assumed a higher temperature. According to this idea we might consider respiration as "serving to the production of heat;" and it was also not absurd to regard some constituents of the food as respiratory food. But when accurate determinations of G. v. Liebig and of Gavarret showed, in opposition to all earlier observations, that the arterial blood is colder than the venous,

the chemical theory of respiration, which through the investigations of Magnus had already begun to give way, seemed to have lost its last support. By the fact mentioned, however, development of heat in the lungs was not excluded: it was seen only that the production is less than the loss. Now the loss is, especially in cold dry air, very considerable; for not only does carbonic acid escape in the gaseous state, but the inspired air is also warmed to nearly the temperature of the blood and at this temperature almost saturated with moisture. If the heat developed here cannot counterbalance this loss, this does not show that heat is not produced. In the first place, the mere solution of the oxygen taken in would afford heat; but, moreover, we may readily assume, that even the loose chemical combination with the colouring matter cannot take place without the development of heat. That in the lungs we have not to do simply with a physical change of gases has indeed, been satisfactorily shown by more than one investigation, carried on in the school of Ludwig. One of the latest, that of Holmgren, affords even the direct proof, that the carbonic acid is expelled from the blood by the oxygen, not as if a vacuum, free from carbonic acid, were in question, but actively; and this is scarcely conceivable without the intervention of a chemical action. What is more, an investigation of Sachs, has recently contributed fresh reasons for the formation of carbonic acid, without the tissues, in the blood itself.

On the other hand we know that, in more than one way heat is indirectly produced. In all parts of the nervous system electro-motor action arises, which is not without influence on further chemical change, since it may even make the dissolved oxygen active and thus promote oxidation, while it also undoubtedly, under the influence of the great resistance presented by the badly conducting tissues, is in great part changed into heat. On irritation, according to Valentin, the development of heat increases in the nerve itself. Also in the muscles electro-motor action gives place to heat. Moreover the giving way of the elastic tension, which remains after each contraction, must be found again as heat (compare the following section). Further, the amount of heat is very considerable which arises from transformation of mechanical work of the muscles. I refer, with respect to this point, in the first place to the heart, the most active of all muscles, producing in twenty-four hours a work of about 86,000 kilogrammeters, which, wholly changed into heat, can warm fully 200 kilogrammes by 1°C ., and therefore all the parts of the body by 3°C . ($5^{\circ}\cdot4\text{ F}$.). The work of the heart, at first in part transferred to the elastic vascular walls, causes the movement of the blood, and the friction connected herewith, consumes the tension, to replace it with heat, which is also the case, in so far as this tension forces the constituents of the blood through the membranes. And if by the friction in the capillaries the oxygen united to the blood-corpuscles may be

made active,* which I have long thought probable, we shall, in its union with other matters find again the work connected therewith as further product of heat, or under another form. I refer, moreover, to the movements of respiration, which are, it is true, slight in comparison to the energy which each time here arises and disappears as tension of the muscles, in order to give place to heat, but which, just like the action of the heart, never cease, and by friction of the air passing in and out, and friction of the passively moved parts constantly convert mechanical work into heat. The same is true of gastric and intestinal movement, and other involuntary contraction of muscles. Finally, in all transitory muscular action, friction must arise, both in the muscle itself and in the articulations, and in general in the passively moved parts; and thus, with consumption of mechanical work, heat is produced.

We may, on the ground of all this, safely assume what may also be more accurately proved by calculation, that the bodily heat is for the most part developed in and through the muscles, principally, as has been said, indirectly, but, as it appears, also directly from the chemical action. This last holds good, in fact, for all parts of the body. Everywhere, where there is capillary circulation, is arterial blood changed into venous, oxygen is chemically combined; and if neither elastic tension nor electro-motor action supervenes, we are justified in inferring the direct origin of heat. Indeed in the high temperature of the blood of the hepatic veins (G. von Liebig and Gavarret), and of the saliva secreted under nervous irritation (Ludwig), we have the direct proofs of development of heat in non-contractile parts. And as in continued muscular work the circulation of the blood and respiration are more active, and evidently a greater quantity of blood is conducted through all organs, we have to expect everywhere an increase of the direct development of heat: in all irritated parts this increased activity manifests itself, after continued work, in the increase of irritation, in pain and swelling.

The conclusion is, that both in the blood and in the tissues, heat is produced directly from chemical energy; but that in addition the electro-motor action in nerves and muscles, the energy of the elastic tension of these last, so far as it is not utilised in mechanical work—and lastly, a considerable part of the mechanical work itself are changed into heat.

IV. PRODUCTION OF MECHANICAL WORK.

As our muscles contract they are in a state to perform mechanical work. This contraction is the result of a change in the molecular

* Experiments of Saintpierre, *Polytechnisches Journal*, B. 162, H. 3, May, 1864, which have reached me while these sheets are passing through the press, seem to prove that mechanical friction really ozonises the oxygen of the atmospheric air.

condition, which depends upon chemical metamorphosis. At the moment when this change occurs, the corresponding contraction is not yet present. The change begins by producing a state as if the muscle was extended, a state wherein the energy consists in the form of an extended elastic body. The contraction itself now follows directly. In this sense Weber is right when he deduces all work of the muscles from elastic forces, which, in their turn, are the result of an altered molecular condition. A correct illustration of what happens here is supplied by a load hanging to a string which becomes shorter by imbibing moisture. The weight is gradually raised, in consequence of the greater elastic tension, which is itself the result of the molecular condition altered by taking up water.

The elastic tension of the contracted muscle cannot, however, be entirely converted into mechanical work. If the load is infinitely great the muscle maintains its original length, notwithstanding the altered molecular condition: of the elastic tension produced nothing is then used for mechanical work. Could the load be infinitely small, the work produced would be, though the load were moved, likewise infinitely small. In both cases, therefore, all mechanical work is wanting. But in any other loading also elastic tension still remains over at the end of the contraction, which is not converted into mechanical work—in general the more the greater the load is. Thus, by removing the load in parts, we see what remains each time rise somewhat higher, and thus with diminution of the tension mechanical work is developed; but finally, even on account of the weight of the parts of the body, elastic tension which can no more be utilised always remains over. The rule is, that the load is put off at a definite height, and as, at the same moment, the muscle relaxes, the just disappearing tension must appear under another form. This can hardly be any other than that of heat. It thus appears that by the relaxation energy is sacrificed, in this sense, that it is not used for mechanical work. But this relaxation is the *conditio sine quâ non* for the continuance of the work. In the relaxed condition alone is it possible that, by a fresh contraction, work should be anew performed. If the muscle continues tense the work ceases. What we here see is perfectly analogous to what the steam engine exhibits in the rising and falling of the piston. In this case, too, the tension remaining in the steam after the raising is sacrificed, and is, as heat, transferred to the cold water which condenses the steam. Now the piston can also again ascend, just as the relaxed muscle can again perform work. But as little as it can be called work when the piston is held above by unaltered tension of steam, so little is permanent contraction of a muscle, which holds a load only at the same height, to be considered as work. Mayer had already fully understood this. Strikingly he says:—"The performance of a man who with great effort holds a weight free, or stands for hours quite unmoved, is exactly—*nil*; the same, or indeed

much more, can be accomplished by a wooden statue." Bécларd^a calls the tension which makes equilibrium with a load, without moving it, *statical*; the movement *dynamical* muscular action. To this distinction we must strictly adhere. Superficially it appears something strange that the tension which is necessary for bearing a load at the same height is not to be called work. The muscles are thereby wearied as well as by raising a load, and the fatigue makes us involuntarily think of and believe in the performance of work. The strangeness, however, immediately vanishes when we take into consideration that in the statical action also work in the muscles is by no means excluded; we say only that there is now no work externally performed, and that we, therefore, must rediscover the whole equivalent of the chemical change which occurs in the muscles, in the body itself, at last under the form of heat. *A priori* we have even no right to set down the metamorphosis as less in the muscle which has only to support a load than in that which has to raise it. If in the first case the mechanical work is wanting, so much the more heat might be developed. In fact, Bécларd found that the skin on the biceps muscle increases in heat in statical more than in dynamical action, and he thought that this difference might be brought into connexion with the equivalent of the work performed in dynamical action. *A posteriori* it appears, however, that in the muscle the change is actually less when no work is performed externally. Bécларd's result is explained by this, that in dynamical action the circulation in the muscles is brisker, and the blood flowing in great quantity carries away the heat which has been developed from the muscle.

The great difference in the metamorphosis in statical and dynamical action is sufficiently evident from their influence upon the factors of the metamorphosis of tissue. Many years ago I instituted investigations upon this point, of which I have as yet published only the results.^b Among these is the fact that in hoisting or lifting a load the pulsations of the heart increase much more in frequency than in mere holding, although the same power which extends the muscle in the first case acts only alternately, while in the second it acts incessantly. The influence on respiration, too, is known. "During a great effort," says Mayer, "it is usual to hold in the breath in order to obtain from the chest a firm support for the motor organs; but with this, however, an increased consumption is not yet necessarily combined. In cleaving wood, going up stairs, &c., no one will be tempted to hold in his breath."

In connexion with statical and dynamical muscular action I formerly, also, investigated fatigue. I found that this is not the same in both

^a De la Contraction Musculaire dans ses Rapports avec la Température Animale. Paris. 1864.

^b Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen. 1859. D. ix., p. 113.

cases. The fatigue dependent on long-continued work yields only slowly; it depends on consumption of matter, and probably, at the same time, products of the metamorphosis of tissue are accumulated in the muscles and nerves. The fatigue caused by holding a load, on the contrary, speedily disappears, even if it were pushed nearly to extremes; and we have, in order to explain this, to think rather of impediments to the circulation, in consequence of pressure on the vessels in the tonically-bent muscle, than of diminished abstraction of products of metamorphosis—perhaps^a also of pressure on the nerves. Even when, as a result hereof, the feeling of fatigue still continues, the muscle has again lost^b its greater extensibility, which remains for a long time after continued work, and therefore its physical fatigue.

A muscle which contracts independently, and thereby moves a load, presents, at the first glance, something mysterious. It is not strange that in former times an attempt was made to explain this by assuming the existence of an equally mysterious force. Now, however, we know that a direct solution of such questions is not to be thought of, that we must have recourse to indirect modes; in other words, that we must investigate all that is in any way connected with the phenomena without previously asking when and how the solution is to follow. And certainly of late years physiologists have not been remiss in this direction. The structure of the muscular fasciculi, the termination of the nerves in the muscles, the elasticity, the production of heat, the electrical phenomena, the sounds even of the muscles have been investigated in the conditions both of contraction and of rest; and in order to explain the mutual connexion of these phenomena an attempt has been made to determine, with accuracy the moment of the rise, and the relative intensity of each phenomenon. The dependence on nervous action, the propagation of this action to the nerves, and further to the muscular fasciculi, the chemical composition of the muscles, the products of their metamorphosis, the influence of contraction on these products, even the blood returning from the muscles, have all been subjected to examination. Many important facts have in this way been brought to light. But the peculiar change of the molecular condition of the muscle, which determines the contraction, is still involved in obscurity. Of the connexion of the phenomena we may give the following sketch:—

^a According to Leber (*Zeitschrift f. rat. Med.*, xviii., p. 262), the irritated (not the resting) muscle is fatigued by mere extension; the fatigue is therefore determined not exclusively by the work performed.

^b The mode of investigation, formerly (*l. c.*) indicated by me, may be found more fully described in Dr. van Mansvelt's dissertation, *Over de Elasticiteit der Spieren* (On the Elasticity of Muscles). Utrecht. 1863. [The English reader will find a translation of the description of this method in a notice of Dr. van Mansvelt's work, drawn up by me for the *British and Foreign Medico-Chirurgical Review*, Vol. xxxiii., p. 439.—TRANSLATOR.]

In normal life the muscle contracts under the influence of the nerves. The action herein excited (voluntarily, automatically, or by reflexion), manifesting itself in a modification of the electrical phenomena (Du Bois-Reymond), is propagated with tolerably great rapidity (amounting, in frogs, to about twenty-eight metres [nearly ninety-nine English feet] in the second—Helmholtz), and, as it seems, with increasing intensity (Pflüger) to the so-called primitive bundles of the muscles, which are to be considered as the terminal organs of the motor nerves. In the condition of rest the muscles have, with very slight tone, persistent elastic tension (Weber), and with consumption of chemical energy by oxidation, an electro-motor action is developed (the cause of the “resting muscular current” of Du Bois-Reymond), and thus mediate, perhaps, also, immediate heat is produced; the muscular sound, too, appears to be present even in rest. The contraction (thickening with shortening), of the so-called primitive bundles proceeds from the points where the nerve fibres are attached with flat expansion mediately or immediately to the muscular substance, and is thence propagated to both sides in each fasciculus under the form of waves, so that a muscular fasciculus is not equally shortened throughout its whole length—in cold blooded animals with the rapidity of about a metre in the second; at the same time, too, the tone of the muscular sound rises. The shortening does not commence directly upon the arrival of the exalted nervous action; there is a latent period of about $\frac{1}{100}$ of a second (Helmholtz), with the commencement of which (von Bezold) an important phenomenon coincides and announces the approaching contraction: this phenomenon is an instantaneous electrical discharge (Meissner) lasting less than $\frac{1}{1000}$ of a second (von Bezold), comparatively weak, but still, probably, equivalent to that of the electric organ of fishes.*

* I here follow Meissner’s view without assuming it to be proved. The case is briefly this—Meissner had found that on artificial compression of the muscle in the direction of its fibres a negative deviation of the needle occurs—the same phenomenon which is observed in active contraction of the muscle. He now thought that in the latter case, too, the change of form, with compression in a definite direction, might, in part at least, be the cause of the deviation. That Du Bois-Reymond had many years previously investigated the influence of extension and compression of the muscle on the force of the current, and announced as his result, that in compression “we most frequently see the current diminish,” appears to have escaped Meissner; however Du Bois-Reymond was far from seeking therein the cause of the “negative deviation.” In order to test Meissner’s meaning, von Bezold determined (from the secondary action in a second muscle), the moment of origin, and also later, by comparison with the effect of artificial currents, the duration, and the course of the electrical change on the surface of a muscle after irritation of the nerve by an unclosing induction stroke. From this investigation it appeared that the change of electricity here occurring, consisting of sudden diminution and restoration, takes place immediately on the arrival of the stimulus in the muscle, and passes off in less than $\frac{1}{1000}$ of a second. As the muscular contraction does not begin until after a latent period of $\frac{1}{100}$ of a second, von Bezold correctly inferred that the change of electricity observed, as preceding the con-

On the occurrence of the shortening, the temperature of the muscle suddenly diminishes (Heidenhain and Solger, Thiry and Meyerstein), probably in consequence of the greater specific heat of the contracted muscle^a; but in reality heat is developed (Becquerel and Breschet, Helmholtz) which, with tetanus lasting some seconds, in spite of the greater specific heat, makes the temperature of the muscle rise above what it originally was. The rise continues after the cessation of the contraction (Thiry and Meyerstein), which is to be ascribed, partly to the return of the original specific heat, partly to the yielding of the elastic tension, further, to the descent of the load just raised by the muscle, and probably to still continued stronger oxidation. The original cooling is greater the greater the shortening (Thiry and Meyerstein); the production of heat appears, on the contrary, to be about proportionate to the work of the muscle performed (the same).

A second phenomenon, connected with the contraction, is the modification in the electrical tension, which manifests itself on the galvanometer as diminution of the muscular current, and is therefore called "negative deviation." This continues so long as the state of contraction lasts; so long, also, as there exist on the surface of the muscle rapid alternations in the tension, proved by the secondary tetanus of a muscle, which by its nerve is in contact with the first. The negative deviation may be

traction, cannot possibly be the result of it. But is the change of current actually the same, which in tetanus produces the known negative deviation of the needle? Is it the disappearance and the return of the current in the muscle at rest? Meissner thinks this may be denied. The change of current, determined by von Bezold, he considers as a positive discharge, such as in tetanus is said rapidly to follow one another. His principal proofs consist in this, that in some few induction strokes of the nerve in the second, which are still insufficient to produce tetanus, in place of a negative a weak positive deviation of the needle is seen; and that also in tetanising the negative deviation is wholly absent, when the muscle is prevented from shortening, —while then precisely the secondary tetanus appeared particularly strong, and justified the inference of the existence of strong alterations on the surface of the muscle. These alterations (the discharges of Meissner) should therefore be distinguished from the negative deviation which follows, as a constant effect, on the compression by contraction.

The great difficulty in Meissner's theory is the negative deviation, which also the nerve-current itself gives on irritation; so far, in fact, nothing is known respecting change of form of the nerve on stimulation.

In thus provisionally adopting Meissner's view I nevertheless reserve my judgment. In no investigation are so many sources of error to be met with as in that of animal electricity. We look with interest for the opinion of Du Bois-Reymond, who has laid the foundation of our knowledge in this department, and more than any one else has taught us to avoid each source of error. Though we may sometimes have to wait for his decision it is sure to come, and to be satisfactory.

^a This explanation diminishes the value of the fact. Quite lately the fact itself (Professor Donders writes) is made very doubtful by new investigations of Heidenhain himself, under whose direction it had been found by Solger, independently of Thiry and Meyerstein.

the result of compression by contraction, which, just like any other compression, diminishes the current of the resting muscle; and, independently of this, in tetanus a series of positive discharges arises, which produce the secondary tetanus, without, in their short duration, being able wholly to counteract the negative effect of the contraction on the galvanometer.

The connexion between the phenomena here described and the contraction, with the work connected therewith, has as yet been but imperfectly explained. The cold, occurring at the moment of the contraction, would readily suggest the idea of the metamorphosis of heat into mechanical work. Against this there is, however, a decided theoretical difficulty; and we might in refutation further state, that, precisely when work is performed, namely, when the muscle is loaded, the negative thermoelectric action dependent on the cold is least—if the consecutive production of heat, which under this circumstance, also, is particularly strong, might not easily conceal the cold which has just been developed. However this may be, there is no reason for assuming that the heat produced should disappear to give place to mechanical work. The electro-motor action rather stands in causal connexion with the contraction, in so far as it determines the molecular condition which produces the contraction and the elastic tension. But our knowledge on this point is very imperfect. We do not even know whether the tension, which is perceptible on the surface of the muscles, with the work which the muscle performs by its contraction, increases or diminishes. Meissner only states that when the muscle is prevented contracting, and therefore performs no mechanical work, the negative deviation, obtained by stimulation, is much less, while nevertheless the alteration of the current ascertained by a very strong secondary tetanus, may be more powerful than when contraction ensues.

Béclard had arrived at the result that the development of heat and of dynamical work in the muscular contraction compensate each other. Of the greater chemical energy sacrificed to the contraction, when it exercises dynamical work, this work should use so much that the heat developed would be not only relatively but absolutely diminished. I have already remarked that, in dynamical action, the blood circulating freely through the muscles carries off much more heat than in statical, and that hence the less elevation of temperature in the investing skin, observed by Béclard in dynamical action, can be explained.

Whilst now Thiry and Meyerstein, in their experiments on the free muscles deprived of circulation, saw an increased production of heat in general combined with increased work, the idea of a compensation of dynamical work and of heat seems to be completely refuted.

The conclusion is this:—An electrical discharge in the muscle, the direct result of the nervous action, leads, with the development of chemical

action, to a rapidly-increasing and again diminishing elastic tension (the contraction curve of Helmholtz). If a number of such discharges follow rapidly on one another, their effects combine each instant to an equal—and thus persistently to an uniform tension. In connexion with the elastic tension electro-motor action and heat arise.

The cause of the elastic tension is not known. It may be situated in the electro-motor action. This passes, however, also partly directly into heat, perhaps also into chemical energy.

It cannot be assumed that heat is changed into electric tension. Heat arises rather as the last form, partly, probably directly, from the chemical energy, partly mediately from the electrical currents and from the elastic tension remaining in the muscle after the removal of the load.

The origin of heat from elastic tension teaches that in the muscle more elastic energy is developed than is employed in mechanical work. Of the mechanical work a further part (that of the heart, stomach, intestines, &c.) passes into heat. *Consequently we recover in the mechanical work yielded externally only a part of the elastic energy produced in the muscles.*

(To be continued.)

Obituary.

It is now our painful duty to add other names to the melancholy list of those whom death has snatched away from among us. Though our losses have not been so numerous during the past as in the previous year, yet in the early part of it there passed from among us Wm. Healy, who was carried off by fever, at the age of forty-five, and George A. Kennedy, who had attained to the more goodly one of seventy-three, and who, though he had retired from the active duties of the profession for some years, still retained a warm place in the affections of his brethren; but as the year closed, death, not yet satisfied, made one fell swoop, and within a few days carried off three more of our colleagues, whose recent loss we still lament and are now called on to record.

EDWARD HUTTON, M.D., F.R.C.S.I.,

Died 24th November, 1865; aged 68 years.

EDWARD HUTTON is a name which shall ever be associated in the memory of those who knew him with some of the noblest attributes of man—with learning, combined with modesty, with firmness, tempered by gentleness, with decision, softened by kindness. DR. HUTTON was born in 1798, and received his education in this city; having determined to study for the medical profession, he was bound apprentice to the late Mr. Robert Peile; and, having graduated in arts in 1817, he obtained the diploma of the Royal College of Surgeons, in this city, at the early age of twenty-one, which he followed up by taking the Fellowship of the same college in 1824, before he was twenty-six years of age; having already, in 1822, obtained the degree of Bachelor of Medicine in Trinity College. In 1842 DR. HUTTON took the full degree of Doctor of Medicine in Trinity College, and in 1852 he was elected President of the Royal College of Surgeons. At a very early age, shortly after obtaining his diploma, he was appointed surgeon to the hospitals of the House of Industry; and those who had the good fortune to study under his guidance will ever remember with gratitude the valuable practical teaching they received at his hands. EDWARD HUTTON was, however, not only a great surgeon he was also a sagacious physician; he was a most skilful operator, and a most successful one too; but he never allowed his better judgment to be overruled by his operative skill, and never permitted himself to be led to perform an operation for the sake of the brilliancy of the achievement; and “*primum non nocere*” was his constant maxim. It is very much to be regretted that Dr. Hutton published so little, as his mind was stored with valuable facts, the accumulation of

years of experience, which would have been of immense service in the cause of science; but with the exception of an article on the treatment of aneurism by compression, which appeared in this Journal in 1843, and some few contributions to the *The Medical Press*, and *The Dublin Hospital Gazette*, he published nothing else so far as we are aware.

DR. HUTTON died on the 24th of November, 1865, he was consequently only sixty-eight; he also, then, was carried away in the full vigour of age, and when, but for the fatal malady which undermined his constitution, we might have justly expected to enjoy the advantage of his knowledge and his skill for many years to come; but Providence willed it otherwise; "the kind teacher, the sound practitioner, the skilful operator, the wise and honourable consultant"^a is gone to his long rest, and we can only mourn for his loss.

DANIEL PAKENHAM,

Died 23rd November, 1865; aged 67 years.

MR. PAKENHAM long held the appointment of State Apothecary. In early life he resided for many years in the Meath Hospital, as apothecary to that institution, and here very many of the most eminent physicians of the present age obtained from him a knowledge of the principles of materia medica, and the elements of practical pharmacy. Here, too, he acquired that practical skill that in after years enabled him to become a most successful general practitioner. In him were found not only professional and scientific knowledge of a high order, but all the qualities that mark the true man; with a chivalrous sense of honour he combined rare earnestness of purpose and self-denial in the highest degree. He died on the 23rd of November, 1865, after ten days' illness of fever, at the age of sixty-seven.

JEROME MORGAN, F.R.C.S.I.,

Died 29th November, 1865.

But the medical obituary, says the President of our College of Surgeons in his address at the opening of the present session of the Surgical Society, makes a still more recent announcement. We have just lost a member who was highly esteemed, and is deeply lamented by all who knew him. The estimation in which he was held by this college is best shown by the length of time he filled the important office of Examiner in Materia Medica. DR. JEROME MORGAN was distinguished for the tact with which he put his questions, and for rectitude and kindness in dealing with the candidate.

^a Words of the President of the Royal College of Surgeons in his opening address.

BOOKS RECEIVED, FEBRUARY, 1866.

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PART I.
ORIGINAL COMMUNICATIONS.

ART. XI.—*On Laceration of the Vagina in the Course of Labour.*
By ALFRED H. M'CLINTOCK, M.D., F.R.C.S.; President of
the Dublin Obstetrical Society; late Master of the Lying-in
Hospital, &c., &c.^a

EXPERIENCE abundantly proves that any part of the genital tract may be lacerated in the progress of labour. The *nature* of the accident is the same, wherever situated, but the causes, the symptoms, the prognosis, and the treatment will materially differ according to the particular situation in which the solution of continuity takes place.

The part most commonly torn is the perineum; next, in the order of frequency, is the lower third or half of the vagina posteriorly, involving, perhaps, the rectum; then the cervical portion of the uterus; next, the upper third, or peri-uterine portion of the vagina; and least frequently the fundus and superior region of the body of the uterus.

Ruptures of the perineum and lower part of the vagina are of common occurrence, easy of recognition, and nearly quite devoid of danger; at the same time I do believe—and I gave some attention to this very point—that these accidents predispose in some measure to uterine or abdominal inflammation, just as any operation

^a Read before the Dublin Obstetrical Society.

in this same region may do with a healthy non-puerperal woman. Their comparative freedom from danger is, in a great degree, attributable to the exemption of the peritoneum from any participation in the laceration or its effects. I say "in a great degree," because rapidly fatal ruptures of the uterus do take place, in which the peritoneum is wholly free from implication; whence we may infer that the danger does not depend solely on the peritoneum being involved, though this adds materially to it.

Whilst the pathology, diagnosis, and treatment of ruptures of the *uterus* are fully described in all the modern English treatises upon midwifery, strange to say no separate or distinct consideration is given to lacerations of the *vagina*; these are just incidentally mentioned, or spoken of as though identical, or nearly identical, in all particulars save situation, with uterine ruptures. Indeed, with the exception of Goldson, whose pamphlet was published in 1787, English authors have taken little or no notice of vaginal laceration. Of continental obstetric authors, however, there are many—and I would mention particularly Boer, Baudelocque, Moreau, Lachapelle, and Danyau—who recognise many of the points of difference between these two lacerations of the genital canal. I cannot, therefore, lay any claim to originality in this memoir. My sole object is to bring before the society, in a brief but connected manner, the pathology, symptoms, and treatment of the accident, together with the history of some examples which fell under my own observation.

The vagina is an organ differing essentially from the uterus, in its structure, relations, functions, and pathology. Hence it is reasonable, *à priori*, to assume that the lesions it sustains in consequence of parturition would have some proper and distinctive characters. I fully admit there is a general resemblance in the symptoms between ruptures of the uterus and lacerations of the vagina, that both are highly dangerous, and that in both classes the primary indication of treatment is the same. Still there are good reasons why we should recognize the differences between them. Accuracy of diagnosis must precede all improvements in practice; and by establishing a clear distinction between these two accidents, and studying each by itself, we shall obtain a fuller and more correct knowledge of the diagnosis and the treatment of both these most formidable complications of childbed.

Lacerations of the vagina—the upper part I mean—are not so frequent a consequence of parturition as are ruptures of the uterus. Let me here make an observation. Collins, and many other

observers, when describing the seat of a laceration, employ the expression, "at the junction of the cervix and vagina," or some phrase of like import. Now, all such cases—their number is but very small—I include under the heading of vaginal laceration, not merely because their claims to being considered uterine or vaginal are equally balanced, but because in some important particulars they more resemble lacerations of the vagina than of the fundus or body of the womb.

To determine the comparative frequency of lacerations of the vagina would be interesting and important; but, unfortunately, most of the published statistics of lacerations of the genital canal are not sufficiently exact in recognizing the site of the injury to furnish data for settling this question. The most extensive collection on record of such cases is that contained in the valuable monograph by Dr. Trask; but, though purporting to be "*On Rupture of the Uterus*," it includes, without distinction, cases of laceration of the vagina. I have no doubt whatever that if practitioners and authors were always careful to ascertain and define the exact seat of laceration, we should find the accident in question to be far more frequent than anyone supposes. Moreau, indeed, distinctly says he could adduce, from his own experience, as well as from the works of authors, very many facts proving that, in the larger proportion of cases, rupture of the vagina has been mistaken for rupture of the uterus.

But, laying aside conjecture, let us see what data we possess to decide this important question. Dr. Collins records 34 cases of ruptures of the uterus and of the vagina; in *eleven* of these the rupture almost exclusively engaged the vagina. Drs. Johnston and Sinclair, in like manner, describe 17 cases, of which *six* were instances of laceration of the vagina only. In my own hospital experience I met with 11 cases of ruptured uterus and *five* cases of rupture of the vagina. Now it is worthy of remark that in the experience of Dr. Collins, Dr. Shekleton (as recorded by Drs. Johnston and Sinclair), and myself, respectively, the comparative frequency of laceration of the vagina is nearly the same. For example, Dr. Collins had 11 cases out of a total of 34, or one-third; Dr. Shekleton had 6 out of 17, a fraction over a third; Dr. M'Clintock 5 out of 16, or a fraction below one-third. To these we may add the cases of Dr. M'Keever, who met with three examples of lacerated vagina and *ten* of rupture of the uterus, or uterus and vagina. Dr. Joseph Clarke, in his report of the practice of the hospital, relates 8 cases

of rupture, *seven* of which were examples of lacerated vagina; and so, most probably, was the other case, which ended in recovery; but as this might be disputed I shall not reckon it. During Dr. Charles Johnson's mastership of the hospital (1840 to 1847) there occurred 20 cases of ruptures of the uterus and of the vagina, and amongst these were *three* instances of undoubted laceration of the vagina at its junction with the os uteri. To sum up, then, we have here a total of 108 cases of uterine and vaginal lacerations—all occurring in the progress of labour—and out of this number we find *thirty-five*, or over one-third, to be ruptures confined to the vagina, or to the vagina and os uteri.

It would hardly be assuming too much if, from this collection of cases, we were to deduce the average frequency of vaginal ruptures as compared with uterine. I know very well that a much larger proportion than three-fourths of all the recorded cases of ruptures are represented to have been of the uterus; but it must be remembered that the older writers were not very precise in their anatomical descriptions; and, moreover, the names which were applied to the different portions of the genital passages were too vague to insure clearness or exactitude. For example, the term "*collum uteri*," which every tyro would now render neck of the womb, was used by Bartholinus, Columbus, and other anatomists, to designate the vagina.

Danyau, writing about the year 1850, was able to collect 17 cases of ruptured vagina, in which the fetus escaped into the abdominal cavity. As this result does not take place oftener than once in every four or five instances, it is plain that the total number of cases of ruptured vagina to be found scattered through the pages of obstetric literature must be pretty considerable. But it would be a very serious error to suppose that all these cases, if collected, would show the actual rate of mortality arising from this accident, or the relative frequency of vaginal rupture as compared with uterine. These are questions which can only be deduced from extensive and complete series of cases, where none are withheld; and the statistics I have given, from the records of the Rotunda Lying-in Hospital, fully answer this requirement, and, therefore, so far as they go, constitute safe and proper grounds to draw conclusions from.

If we look to the relative thickness of the uterus and of the vagina at the period of labour, it does at first seem a little strange that the former should so much more frequently be the seat of

aceration. The wall of the vagina when much distended, as it is by the fetus, is really very little thicker than chamois leather. I was greatly struck by this in a recent case where I had to remove an incarcerated placenta. The os uteri offered considerable resistance to the passage of the hand, the vagina, in consequence, being put very much on the stretch; and so distinctly did I feel the sacrum and the bodies of the lumbar vertebræ through the distended vagina, that for a moment I scarcely conceived it possible my hand was not fairly in the cavity of the abdomen.

Although, however, the vagina be considerably thinner than the uterus, yet it is comparatively stronger, by virtue of its dense fibro-elastic external coat, to which the uterus possesses nothing analogous in point of structure. It is highly probable, too, that in many cases, especially amongst pluriparæ, the uterus, when occupied by the fetus, and its fibres in a state of relaxation or extension, actually possesses very little more of thickness than does the vagina. Meigs supposes that in most cases of rupture the tear commences in the posterior wall of the vagina, near to the cervix, from its remarkable thinness in this situation. But however probable this might appear from *à priori* reasoning, still it is not at all in accordance with the results of experience, as in a very large majority of the cases of utero-vaginal laceration—not to mention the cases of purely uterine laceration—the rent is situated at the side, and not behind.

Clinical observation teaches us that any part of the vaginal canal may be torn except the lower portion of the anterior wall, which part, though the most common seat of sloughing, was exempt from laceration in all the histories I have perused. The nearest approach to a rent in this situation was a case recorded by Drs. Johnston and Sinclair, in which there was a transverse slit in the base of the bladder, just at its relation to the vagina, and apparently not engaging the peritoneum. In the upper peri-uterine portion of the canal rupture occurs, in front and behind, with pretty nearly equal frequency. When situated anteriorly the bladder is sometimes implicated in the laceration, of which the case just alluded to is an example. The same also occurred, though to a far greater extent, in Goldson's, and one of Dr. M'Keever's cases.

In a considerable number of cases the laceration is situated somewhat more to one side or the other. But wherever it may be it almost invariably takes a circular direction, often extending through one-half, and sometimes three-fourths, the circumference of

the canal. On a few rare occasions the vagina has been almost entirely detached from the uterus. In Dr. M'Keever's twelfth case this was well seen. "The body of the uterus," he says, "was nearly altogether separated from the vagina, being merely retained by a slender thread on the left side." The tear once begun is easily enlarged, either by the continuance of the pains or by incautious attempts at artificial delivery with the hand or with instruments. Of the cases where the rent was chiefly at the side of the vagina the left was more frequently engaged than the right. It is just possible that the laceration may take a longitudinal direction in the posterior wall. Again, it may have the appearance of a somewhat round aperture, as in one of Collins' cases. It is deserving of notice that in nearly all cases where the laceration was of spontaneous origin—*i.e.*, not the result of manual or instrumental interference—it has taken more of a circular than longitudinal direction. In uterine lacerations, on the contrary, the prevailing direction of the laceration—except, perhaps, where the os alone is engaged—is more or less longitudinal.

Whether situated in front or behind, in nearly every instance the laceration has extended through the vagina and into the peritoneal cavity. Dr. Churchill alludes to a case of anterior laceration of the vagina, in which he supposes, and with much probability, that the serous coat did not give way till just before her death on the fifth day. In one of Dr. Collins' cases there existed a rent in the same situation, the peritoneum over it being entire. Another case is related by him of extensive utero-vaginal laceration posteriorly, and the serous membrane covering the rupture was intact.

The following may be mentioned as the most influential predisposing causes of vaginal laceration, *viz.*:—

1. Disease of the vagina.
2. Disproportion between the size of the child and the capacity of the pelvis.
3. Osseous irregularity on the internal surface of the pelvis.

1. The diseased condition of the vagina which has most commonly led to its rupture was narrowing, or contraction with cicatrices, the result of former inflammation and sloughing. Both Dr. Collins and Drs. Johnston and Sinclair give examples of laceration arising out of this vicious state of the vagina.

2. Decided contraction of the pelvis was observed in only a very small proportion of cases, whilst in a smaller number the disproportion was due to a hydrocephalic condition of the fetus.

3. Denman was the first to suggest that the attrition sustained by the uterus (or vagina), between the fetal head and an abnormally salient promontary of the sacrum, might cause it to lacerate. Dr. Collins narrates a case where the solution of continuity in the vagina corresponded very accurately with the unusually projecting promontary of the sacrum; and Drs. Johnston and Sinclair, when describing the *post mortem* appearances in their eighth case, observe:—"A laceration of the vagina was found to exist, leading from a point anteriorly towards the left, and about four inches in extent. The pelvis showed no diminution, but on the internal surface of its anterior wall a prominent ridge was observed, about an inch long, and running in a direction from the centre of the junction of the pubic bones downwards and to the left."

In every instance that I know of, where vaginal laceration was spontaneously produced, the head was the presenting part. A case is recorded by Dr. Collins which forms a partial exception to this statement. Here the breech presented and the delivery was easily effected; but the uterus was extensively torn along with the vagina, so that I do not reckon this case amongst those of vaginal laceration. In not a few cases there was presentation of the upper extremity, and it was in the efforts to rectify this malposition of the fetus that the practitioner was so unfortunate as to lacerate the vagina.

The number of the labour does not seem to have much influence upon the production of the accident, beyond this, that it is proportionately much less frequent in first than in second, third, or fourth labours respectively. Of forty-eight cases where this circumstance was noted, I find *six* were primiparæ and *forty-two* pluriparæ, viz.:—*Ten* in the second labour, *eight* in the third, *nine* in the fourth, *two* in the fifth, *five* in the sixth, *three* in the seventh, *two* in the eighth, and *one* in the ninth, tenth, and eleventh labours respectively. As regards the sex of the child the same law holds good as in ruptures of the uterus, the majority being males; out of twenty-seven cases *fifteen* of the children were boys and *twelve* were girls.

In regard to the mode of its production it is easy to comprehend that inordinate uterine action must play the most important part; but powerful uterine contractions alone will hardly cause laceration of a healthy vagina with a normal pelvis, unless there be some unnatural roughness or projection on the interior of the pelvis. But if there be any obstacle or hindrance to the advance of the fetus, after its head has cleared the os uteri, under these circumstances,

the vagina will have to sustain a degree of traction or extension exactly equal in amount to the strength of the pains. Hence it is often found in practice that for the production of the laceration, uterine action and obstruction from some cause or other have concurred.

In almost all cases the head had not only entered the pelvis, but in the majority of them was fully engaged in its cavity at the time of the rupture occurring; and the plain inference to be drawn, I think, from these facts is, that contraction of the brim of the pelvis is not by any means so influential a cause of laceration of the *vagina* as it is of rupture of the uterus, especially of its neck. Simple over-distension of the vagina, as from the forcible extraction of an emphysematous child or hydrocephalic head, might cause it to burst. In this case the greater the capacity of the pelvis the more risk would there be of rupture. On very many occasions the vagina has been torn by attempts to force the head into the uterus for the purpose of turning the child, or of rectifying some real or fancied malposition of the head. It has also been lacerated by the premature or unskilful use of the forceps.

Many other causes for rupture of the vagina are laid down by authors, such as violent movements of the fetus, obliquity of the uterus, attitudes of the mother, &c., &c., which I do not think it worth while to stop and examine, as they seem to be drawn more from theoretical consideration than clinical observation. That mechanical violence, inflicted on the abdomen during labour, might be competent to tear the vagina not only seems highly probable but is confirmed by a case related in the work of Drs. Sinclair and Johnston.

With respect to the etiology of rupture there is one point on which the vagina and uterus stand in marked contrast, namely, that whilst the latter is frequently burst by its own active contractions, the vagina never is, nor can be.

Though I have thus described many causes for the production of this accident, it must still be confessed that in no small number of cases there was really no assignable cause for its occurrence—no pelvic deformity, no malposition, no disproportion, no violent uterine action—so that we have yet much to learn upon this point in the history of the lesion. Some of the older writers regard vaginal laceration as a more fatal accident than uterine laceration. This opinion was not deduced from any clinical facts, but seems to have been based on the observation that a rent in the uterus underwent

a diminution of size in consequence of the contractions of the organ; whereas the absence of any such contraction in the vagina allowed the laceration of this canal to retain its full size. The reasoning seems plausible, and the facts on which it rests are undoubted; nevertheless it is a fallacy, and supplies an apt illustration of the danger of *à priori* conclusions in medicine.

The *prognosis* to be formed of laceration of the peri-uterine portion of the vagina must always be grave, but yet qualified according to the circumstances of the particular case. Let us first inquire what is the average mortality in the general run of cases of this accident, and then examine what are the special conditions which should guide us in forming a judgment upon any individual case. I am sorry to say we do not possess any adequate trustworthy body of facts from which a conclusion might be drawn respecting the former of these questions, *i.e.*, the average mortality among cases of this accident. Dr. Trask's statistics we can not safely use for this purpose in consequence of his observing no distinction between cases of rupture of the vagina and rupture of the uterus. A collection, even, of all the published cases of ruptured vagina, valuable though such would be, would not supply just data to go upon, and for the simple and obvious reason, that they do not represent *all the cases* that have actually occurred, but consist chiefly of the exceptional ones—picked cases—those, namely, which were remarkable from the fact of the patient recovering.

To form any, or the barest, approximation to the truth upon this important point, therefore, I am obliged to go upon the data supplied by the experience of the Rotunda Lying-in Hospital, though fully aware that these data are not by any means sufficiently extensive to yield the true average.

This experience, then, furnishes thirty-five cases of laceration of the vagina, and of these four patients recovered; whilst of the seventy-three remaining cases, where the rupture was exclusively or chiefly confined to the uterus, only *three* recovered, *one* under Dr. Clarke, *one* under Dr. M'Keever, and *one* under Dr. M'Clintock; but it is very questionable whether the first and last of these cases should not be included in the former category, namely, lacerations of the vagina. Leaving them as they are, however, there is a wide difference between four recoveries out of thirty-five cases and three out of seventy-three—in other words, between twelve per cent. and four and a quarter per cent.

It would be highly interesting and important to know the actual

rate of mortality among cases of rupture of the uterus; but in truth we do not at present possess the requisite data for determining this point. Dr. Trask's most valuable and elaborate statistics cannot, for reasons already mentioned, yield a correct solution of the question.

My own researches lead me to believe that rupture of the uterus above the os is far more dangerous than laceration of the vagina, and that a large proportion of the cases published as examples of recovery from rupture of the uterus were in reality examples of laceration of the vagina, or of the vagina and os uteri. This idea is not original; Professor Boer of Vienna, Professor Dubois and M. Danyau of Paris have expressed a similar opinion. Danyau collected seventeen cases of ruptured vagina, in which the fetus escaped into the cavity of the belly, and of these cases *four* ended in recovery. I have collected, from easily accessible sources, fifty-one cases of laceration of the vagina, including *thirteen* instances of perfect recovery.

From the foregoing facts and observations I think it is justifiable to draw the conclusion that the danger to life from vaginal laceration is very much less than from uterine laceration. A most interesting question here presents itself. What is the cause of this difference? Why should rupture of the uterus be so much more dangerous than that of the vagina? In very many fatal cases (Trask mentions twelve) of uterine laceration the peritoneum is unbroken; and of the *thirteen* recoveries from vaginal laceration, in my collection, *twelve* had the peritoneum involved in the tear. We may safely assume, therefore, that the implication or non-implication of the serous membrane has nothing whatever to say to this difference. Mr. Goldson, who has the merit of being the first English writer to draw attention to the subject of vaginal lacerations, assigns as a reason of their being less dangerous than uterine lacerations that the delivery of the child is accomplished with so little difficulty. This refers, I presume, to the instances where the child has escaped into the cavity of the belly; of such cases, and of them only, the assertion is strictly correct.

If we look to the comparative importance of the two organs, as indicated by their organization and by the effects of their diseases, especially puerperal diseases, we must at once admit that the uterus stands much higher in the scale than the other.

Again, the changes which are set up in the uterus, immediately on the completion of the act of parturition, are most unfavourable

to the repair of any injury or wound of its structure. A general disintegration and removal of the uterine structure is going forward, attended by a degradation of its tissue. This series of processes has been well described by Dr. C. West, and is looked upon by him as an influential cause of the great mortality following the Cæsarean operation.—*Med. Chir. Trans.*, Vol. XXXIV.

In forming a prognosis upon any individual case the exemption or implication of the peritoneum is a circumstance which, if known to us, should have considerable weight. The cases in which it escapes are extremely rare; and when the tear engages the posterior region of the peri-uterine vagina it is hard to conceive it possible for the serous cavity to avoid being opened. I only know of three cases where the peritoneum was stated not to be involved, and one of these recovered. Dr. Collins gives a case where the rent was situated anteriorly, not involving the peritoneum, and yet the woman died almost immediately upon delivery. We have already seen that in several of the recorded fatal cases of uterine rupture the serous covering of the womb was intact. All these facts would seem to show that whether the peritoneum be engaged or not has comparatively little influence upon the result.

Another important circumstance to take into our consideration, in reference to prognosis, is the amount of hemorrhage. A great source of danger in all these cases is the extravasation of blood into the peritoneal cavity. The experience derived from operations upon the abdomen gives the strongest confirmation to this.^a We cannot always know, however, whether any or much blood be effused internally, as the quantity of external discharge is not an infallible index, though supplying ground for probability.

Of course the greater the extent of the laceration the greater, *cæteris paribus*, will be the danger.

The severity of the constitutional symptoms immediately supervening on the accident deserves careful consideration in estimating the chances of recovery. A great amount of shock would indicate either an extensive injury, or large effusion, or a highly irritable

^a Some surgeons are of opinion that effused blood has not, by itself, an irritating effect upon the serous membrane, but that it is the presence of air along with the blood that does the mischief, by inducing decomposition of the latter. Decisive facts are wanting to clear up this point, as the circumstances under which blood most commonly enters the serous sac will also allow the admission of air. Even should the opinion of these surgeons be confirmed, the statement in the text will not thereby be affected, as in nearly every instance of vaginal laceration there is almost nothing to prevent the admission of air.

state of constitution, rendering the patient a bad subject for any accident or operation. The almost total absence of shock in the two cases of recovery from extensive vaginal laceration recorded by Dr. Ross, of Hamburg, was a very remarkable feature. These two cases occurred in the same woman, in her fourth and fifth labours.

Whether the rent be situated anteriorly or posteriorly does not seem to have any influence upon the result.

The length of time elapsing between the occurrence of the accident and the delivery of the patient is an important element in our calculation. The shorter this interval the better for the patient.

The escape of the child through the rent into the cavity of the belly must be regarded as an aggravating circumstance; and yet Danyau's collection of cases, already alluded to, in all of which it occurred, show good results, namely, four recoveries out of seventeen cases.

I need hardly add that the presence of an unreduced portion of intestine in the vagina would almost deprive the patient of any chance of recovery. In one such case (recorded by M. Percy, and mentioned in Baudelocque) the patient died the third day, with all the symptoms of strangulated hernia. On the other hand, to show us we should never despair, there is the memorable case of Dr. M'Keever's, in which nearly four feet of intestine prolapsed through the rent in the vagina, and eventually sloughed off; and yet this patient recovered.

The train of symptoms, when fully developed, which vaginal laceration produces is nearly the same as that following upon uterine rupture. There is no symptom peculiar to the one or to the other accident, so far as we at present know, and, therefore, no single symptom pathognomonic of either. Nevertheless, as regards the frequency and the prominence of individual symptoms, experience does show a considerable difference between the two lesions.

Before going further I should remark that there are no premonitory symptoms, properly so called, of laceration of the vagina. Of course we might apprehend its occurrence in a patient whom we know to have a diseased vagina, or a narrow outlet, or any osseous projection of the pelvis; and, in any case, the continuance of powerful expulsive pains, without advance of the head, after this had entered the pelvic cavity, should suggest to our mind the possibility of the vagina giving way. If one or other of the above

predisposing circumstances were present in the case the possibility would be changed into a strong probability.

Let us now briefly review the *symptoms* which may follow upon the accident in question. The number and severity of these will, as a general rule, bear a proportion to the extent of the injury and to the constitutional irritability of the patient. The latter has, I believe, the more influence, a great deal, of the two, in the production of any shock as a consequence of the lesion. It is reasonable to suppose that the exemption of the peritoneum from implication in the laceration would tend to lessen or avert the disposition to nervous shock. Certainly in two out of three cases where the peritoneum was not involved there was a total absence of symptoms of collapse. Even where the serous membrane is extensively torn there may be comparatively no indication of shock to the system, as in the following case of my own:—

CASE I.—A short plethoric woman was admitted, in labour of her first child, in the afternoon of 25th December. The dilatation of the os uteri was not completed for nearly thirty hours; but the second stage was short, and a dead female child was born, by the natural efforts, at 5 a.m. of the 27th. When seen a few hours afterwards, at morning visit, her pulse was observed to be very frequent, but there was no other symptom to arrest attention. At 7 p.m. of same day the pulse had risen to 126, and was extremely weak; the face was rather congested; the tongue dry and coated; the belly full and tympanitic, and extremely tender to pressure; the respiration laboured. The woman was evidently in the most imminent danger. I should remark that she had no vomiting then, or at any time previously since the commencement of her labour. She expired at 8 a.m. of the 28th. There was a good deal of bloody effusion into the abdomen, with marks of incipient peritonitis. In the posterior part of the vagina, just below the os uteri, was an extensive laceration involving all the tissues of the canal, and engaging nearly three-fourths of its circumference. The perineum was torn, and the vulva was rather sloughy.

On the other hand, with no greater amount of injury than occurred in the case just related, the effect on the vital powers may be so overwhelming as speedily to extinguish life, of which the following case (which occurred when I was a pupil at the Lying-in Hospital, in Dr. Charles Johnson's mastership) is a good example:—

CASE II. occurred at the hospital in the year 1841. The woman was in her fourth labour, and before admission had been eighteen hours ill. The arm and shoulder were presenting; turning was effected without any unusual difficulty, and a living boy extracted. The patient at once began to sink, and expired in two hours, despite of everything that was done to sustain and rally the vital powers.

At the *post mortem* examination an extensive laceration was discovered in the posterior part of the vagina, and just at its junction with the os uteri.

Doubtless the operation of version here contributed somewhat to the shock under which the patient sank.

Setting aside exceptional cases, then, I think it will hold good as a rule that the symptoms of collapse are not developed so soon, or in such a marked manner, after vaginal laceration as they are after uterine rupture. I only speak of what occurs at the outset. Of course, with the lapse of time, and if the woman be not delivered, it is to be expected that marked indications of depression of the vital powers will present themselves. Thus, in the following case there was an interval of nearly seven hours from the occurrence of the laceration until the woman was brought into the hospital, which is sufficient to account for the symptoms of prostration being so decided at the time of my seeing her:—

CASE III.—This woman fell in labour of her fourth child on the 22nd October, 1859. Early in the morning of the 23rd she was seen by a pupil, who, finding her case dangerous, and her circumstances the most wretched that can well be conceived, brought her off to the hospital in a car. I first saw her at 8 a.m.; she was then in a state bordering on collapse, with a scarcely perceptible pulse, cold surface, pinched face, some vomiting, and slight red discharge from the vagina. She complained of great pain in the lower belly, with extreme tenderness on pressure; and she said that the labour pains had suddenly ceased between 1 and 2 o'clock, a.m., since when the soreness of the belly had come on. The fetal heart was nowhere audible; the head had partially descended into the pelvis, and lay transversely, midway, in fact, between the third and second positions. The proper uterine tumour, hard and defined, could be felt at the level of the umbilicus; above and behind it a movable tumour could just be distinguished, which I took for granted was part of the fetus; but I could not minutely examine it owing to

the extreme tenderness of the abdomen. There had been some vomiting before her admission to the hospital. I delivered by perforation; the child, a girl, was evidently dead for some hours. The placenta came away immediately, with slight traction of the cord. She was very weak after delivery, but rallied in the course of a few hours, under the diligent use of restoratives. Vomiting and debility were the prominent symptoms during the four days that she survived.

Autopsy.—Extensive peritonitis, with large quantities of lymph; uterus pretty well contracted; the vagina separated from its uterine attachment, posteriorly, by a large transverse rent about four inches long; the conjugate diameter of the brim measured three inches and three-quarters.

I have no doubt whatever it was the fetal buttock or lower extremities that we felt before delivery, above and behind the uterus, as the same has been noted by other observers in cases where, as here, the head was still in the pelvis. Bearing in mind the position of the rent, I confess I cannot clearly comprehend how the trunk of the child gets out of the uterus into the abdomen, its head remaining all the while in the pelvis. To pass through the rent head-foremost would be intelligible enough; but in the cases I speak of the head does not change its position, or is the last part to recede out of the genital canal.

By some it is supposed that the severity of the constitutional shock depends on the amount of hemorrhage resulting from the laceration. Though I would not altogether exclude hemorrhage from having some effect this way, yet hemorrhage in any considerable quantity is rarely present; and, besides, both analogy and experience justify us in considering that its influence must be comparatively very small.

Vomiting is by no means constantly present in vaginal laceration, and the matter ejected very rarely possesses the coffee-grounds character. This latter character was wanting in each of my cases. Nevertheless vomiting is a symptom whose appearance in the second stage of labour should always put the attendant on his guard, and awaken a suspicion of rupture somewhere.

The same may be said of *hemorrhage* coming on after the woman has entered the second stage of her labour. When the consequence of rupture the sanguineous discharge is seldom profuse, or sufficient to occasion, by its quantity merely, any alarm for the safety of the

patient. It is a symptom pretty generally present in ruptures, and more so, perhaps, in vaginal than in uterine ones. The contrary to this might have been expected from the greater size and number of the uterine vessels. But the vagina is surrounded by a venous network, capable of yielding blood for a considerable time, in consequence of the entire absence of valves, and of any special arrangement (such as the uterus possesses) for compression of the bleeding vessels.

More or less complete *cessation of the labour pains* occurs in most instances of lacerated vagina. In a few, however, the uterus continues to act, assisting the removal of the fetus, or even effecting its expulsion as well as that of the placenta. This actually took place in four out of the fifty-one cases which I have tabulated. One of these occurred under my own observation, and was the first case I related; another occurred to Dr. Collins; a third is recorded by Dr. Ingleby, and a fourth happened in the practice of Smellie. The very fact of the escape of the fetus through the laceration—an occurrence very far from uncommon—supposes the presence of uterine contractions of moderate strength, at all events.

CASE IV.—This was a large stout woman, in her second labour. The os uteri was fully dilated, and the membranes ruptured, about midnight, after which time the uterine contractions became very strong and frequent, and she herself was restless and unmanageable. When I saw her, at 9.30 a.m., the pulse was 112; there was a large tumour on the head, which had descended into the pelvis, but not entirely, and was firmly wedged there; the pains were apparently most powerful, but making no impression whatever on the fetal head; she was very restless, and the sides of the belly were tympanitic. The most careful auscultation failed to detect any trace of the fetal heart.

I felt that she should be delivered without delay, though I freely confess I did not suspect the existence of any laceration.

After perforation the head was brought away without difficulty; the child, a boy. On dividing the cord the blood contained in it was found to be coagulated. The pulse continued frequent from the time of delivery, and the same evening she complained of pain in the belly, which was full and tender. At seven o'clock of the next morning, after a restless night, she suddenly began to sink, and expired within an hour.

Autopsy.—Well-marked peritonitis, with lymphic exudation;

serous and sanguineous effusion in the lower belly; in the posterior and left side of the vagina, rather low down, and freely communicating with the peritoneal cavity, was an extensive laceration. The pelvis appeared of normal size.

This case was remarkable, first, for the absence of all the characteristic symptoms of laceration; and, second, for the sudden and unexpected manner of her death.

I have related the case chiefly to show that the pains may go on after the occurrence of a vaginal laceration; but it also illustrates the difficulties which occasionally beset the diagnosis of the lesion in question. In this respect the case bears a strong resemblance to the first one detailed.

Contrary to the opinion of Churchill and some other authors, *recession* of the presenting part does take place, slightly, in the greater number of cases of vaginal laceration, and totally in those cases where the fetus slips into the sac of the peritoneum. This last symptom (the escape of fetus into belly) is much less rare in vaginal than in uterine lacerations. For this two reasons may be assigned—first, the continuance of uterine contractions, and second, the uncontracted patulous state of the rent in the vagina. The same reasons will serve to explain why the placenta on some occasions follows the child into the abdominal cavity.

Where the fetus has partially or entirely escaped out of the uterus and into the belly, its limbs and body are readily distinguishable through the abdominal parietes, unless these be enormously loaded with fat, or the belly become very tender. Sometimes the contracted uterus may, by the same mode of examination, be felt and recognized with great ease. Short of feeling the laceration itself, there is no more demonstrative proof of its existence than the symptoms just mentioned. But it is only within a limited, though variable, period that the child can be thus distinctly recognized. In the course of a few hours the belly becomes swollen, and tense, and tender, and the patient cannot tolerate any manipulation of the part. Under these circumstances it would be very difficult, or impossible, to discern the limbs of the extra-uterine fetus.

Prolapse through the laceration, of intestine or omentum, into the vagina, or even its protrusion externally beyond the vulva, is not an unfrequent complication of lacerations of this canal, though an extremely rare consequence of ruptures of the fundus or body of the uterus. This may, in great measure, be accounted for by the

uncontracted state and thin edges of the vaginal laceration. Of the fifty-one cases I have collected of vaginal laceration, there was a hernia of the intestine or omentum in *eleven*. This hernia in *two* instances actually occurred before the removal of the fetus; and in *one* most remarkable case (that recorded by Dr. M'Keever) it took place on the fourth day after delivery.

The position of the intestines above and behind the gravid uterus prepares us for what experience shows to be the fact, namely, that the prolapse of intestine is more apt to take place when the laceration is situated in the posterior wall of the vagina, though it has occurred when the rent was in front.

Cessation of the fetal heart's sounds—a useful corroborative sign of rupture of the uterus, and one to which I particularly drew attention many years ago^a—would seem to be of equal value in ruptures of the vagina; for, of twenty children delivered naturally or by non-destructive operations, after laceration of the vagina, only one was born living. But this instance proves nothing, as the child was delivered by the very act—turning—which tore the vagina; so that the interval between the two events, viz., the laceration and delivery, could have been only a few moments. I think it very probable, however, from the lesser intensity of the symptoms in laceration of the vagina, that the death of the fetus does not supervene quite so soon as after rupture of the uterus. Unfortunately facts are still wanting to enable us to determine this important and interesting point.

Subcutaneous emphysema of the lower belly is a rare, but, when present, a reliable symptom of ruptured uterus.^b I cannot say whether it ever takes place after vaginal laceration. I have not myself met with it, nor seen any mention of it in published cases; and I am disposed to think, for anatomical reasons, that it is much less likely to follow vaginal than uterine ruptures.

We now come to speak of the *treatment* of laceration of the

^a See a "Memoir on the Use of Auscultation in the Treatment of Labours," published in Vol. iv. of this Journal (August, 1847).

^b The first time, I believe, that emphysema of the hypogastrium was observed as a symptom of rupture of the uterus was in a case which fell under my care at the Lying-in Hospital, in the month of August, 1855, and of which an account was published in the number of this Journal for November, 1857, page 450. Since then the presence of this symptom has been observed by others. Dr. H. G. Croly, of this city, has published a remarkable case of laceration of the bladder, from fracture of the pubis, in which, reasoning from analogy, he was led by the presence of this symptom, and this only, to diagnose the fatal injury to the viscus. The accuracy of the diagnosis was verified at the *autopsy*.—*Vide Medical Press* for March 9, 1859.

vagina, and I venture to affirm that if there be a point of practice on which medical men are unanimously agreed it is this, that immediate delivery should be resorted to once it is ascertained, beyond a doubt, that the vagina or the uterus has been lacerated in the progress of labour. This may be laid down, then, as an incontrovertible axiom. The only conceivable exception to it is where the patient happens to be so prostrated as to seem unable to bear the shock of immediate delivery; here it might be necessary to delay the performance of the operation for a short while, till she was somewhat recruited by the administration of stimulants.

There are four ways by which delivery has been effected in the class of cases before us, namely, the forceps, the crotchet, version, and gastrotomy. Upon each of these I shall offer a few, and only a few, remarks.

If the child have escaped into the abdomen, or if the head have receded out of the pelvis, the use of the forceps is impracticable. Even in the more numerous class of cases where the head remains still in the pelvis, the forceps is rarely the most judicious mode of delivery, for this reason, that there often exists some disproportion between the head and the pelvis; and also because, as we have already seen, the fetus rarely, if ever, survives the laceration for many minutes. Where proof is afforded of the child's actually being dead, the forceps should never, in this or any other class of cases, be preferred to the crotchet as a means of delivery, inasmuch as the latter (crotchet) is the safer mode for the mother.^a Among my cases of laceration of the vagina was one delivered by the forceps, not on account of the laceration, whose existence, indeed, was not suspected at the time, but for convulsions:—

^a I am anxious to guard against the above observation being misunderstood, or its application carried too far. What I contend for is, that delivery by the crotchet is, *cæteris paribus*, a safer operation for the mother than delivery by the forceps; and I am fully convinced that reason, experience, and statistics rightly used, all concur in establishing this position. It is of great practical importance to have this question clearly determined, in order to guide us as to the mode of delivery in all those cases—and they form a numerous group—in which the fetus is dead.

In thus claiming for this operation almost the only merit which belongs to it, let it not be supposed that I am undervaluing the forceps. Quite the reverse. Craniotomy should never be an operation of *election* if the fetus be living. Any evidence of the child's vitality introduces into our calculations a new element which forbids all comparison between the crotchet and the forceps, and renders the former wholly inadmissible, except as a last alternative, and when no other resource is left us of saving the life of the patient. Here it truly is an operation of *necessity*.

CASE V.—This was a large robust woman, in labour of her first child, and admitted at 4 p.m. At 7.30 the head was descending in the pelvis and the fetal heart was audible, but there was occasional vomiting. At 10 p.m. a severe convulsive fit occurred, whereupon she was immediately chloroformed, and delivered by the forceps of a dead male child. I was not present at the delivery, but I believe it was effected with ease. There was some post-partum hemorrhage, and at eleven o'clock she had a second fit. The next morning she was heavy and stupid, with a slender weak pulse, and frequent sickness of stomach. The existence of a small thrombus was discovered in the left nympha. At 5 p.m. of this day she had a third fit. She regained intelligence, but died on the fifth day, with obscure symptoms of metro-peritonitis. Curious to say, her pulse was never above 110, and the day she died it was only 88. She could not be persuaded to take the stimulants which were ordered for her.

The *autopsy* revealed peritonitis, and a sloughy condition of the interior of the uterus, which contained a portion of the membranes in a fetid state. An irregular aperture was found in the vagina, very low down, towards the right side, and rather behind. This rent did not communicate with the peritoneum

It may be supposed that the laceration here was caused by the forceps. I do not think so, however, and for the following reasons:—

1. The patient had vomiting in the second stage of labour.
2. The child was quite dead at birth, though ascertained to be living two and a-half hours previously.
3. The laceration was not situated in the place most likely to be torn by the unskilful use of the forceps.
4. The operator was not a novice, but one accustomed to the use of the instrument.

This is the only case I have met with where the peritoneum did not participate in the laceration. Of the two very grave accidents which complicated her labour, namely, convulsions and lacerated vagina, the former had the greater share, I think, in causing her death. In one of Collins' cases, also, the same double complication occurred in a primipara, and with a like result.

Whilst speaking on the use of the forceps I must beg leave to give the particulars of another case, as it helps to illustrate this part of my subject.

CASE VI.—A woman was admitted at midnight, in labour of her eighth child. At 6 a.m. the os uteri was fully dilated and the pains regular. About seven o'clock there was a cessation of the labour pains, and a short time afterwards she vomited. As she did not make any complaint these symptoms attracted no particular attention, till she was seen at nine o'clock by Dr. Jennings, then senior assistant, who at once suspected that some laceration had taken place. Her pulse was now rapid and weak; there was vomiting, and some discharge of blood from the vagina; the fetal heart was wholly inaudible; there were no labour pains present, and the limbs of the child could be felt in the abdomen; the head was in the pelvic cavity. Being myself from home, Dr. Shekleton saw this patient, and at once proceeded to deliver her with the forceps, but in the attempt to do so the head receded quite out of reach; whereupon he introduced his hand, and extracted the child by turning: the placenta soon followed, and there was no hemorrhage. She was very slow in rallying, and was greatly annoyed with vomiting and meteorismus during the next three days. On the fourth day the belly was very much distended, and an enema was given in the hope of lessening this tympanitis. The bowels were acted upon by the enema, and at the same time a large quantity of grumous blood was discharged from the vagina. She died on the fifth day.

At the *autopsy* we found peritonitis, and much extravasated blood in the belly; the uterus was well contracted; there was an extensive laceration in the upper and posterior part of the vagina; this was rather to the left side, and took a somewhat oblique direction, so as to engage the os uteri in a slight degree; the pelvis was well formed, and there was no trace of any pre-existing disease of the vagina

The crotchet has, mechanically speaking, a wider range of applicability than the forceps; and, for reasons already assigned, is more frequently to be preferred in the cases under consideration. The introduction of the perforator requires some caution, else the head may be pushed out of the pelvis, which would probably necessitate delivery by turning. To avoid this we should, if possible, select a fontanelle where but little force would be required to penetrate, or fix the point of the instrument on a spot of the head as near to the side of the pelvis as possible, so that the opposite side may form a point of resistance. A slight curve in the extremity

of the perforator will here be of assistance to us in avoiding upward pressure.

If neither the forceps nor the crotchet be deemed capable of effecting delivery our next alternative is *turning*. The particular circumstance which generally obliges us to resort to this mode of delivery is the partial or complete recession of the child out of the vagina. Should the entire fetus have passed into the belly, it may still be followed by the hand, and extracted without any great difficulty, though much care is requisite to avoid drawing down along with it some of the intestine or omentum. This caution is the more needful if the rent be situated in the posterior part of the vagina. Though several hours may have elapsed from the moment of the laceration we are not, on this account, to be deterred from pursuing the fetus with the hand, and endeavouring to withdraw it through the rent, as such a proceeding is not only warrantable but generally quite feasible.

For example, in a case of my own there was an interval of four hours between the occurrence of the accident and the performance of turning; in Mr. Ross' case there was a like interval (this patient recovered, moreover); in a case of Collins' the interval was twelve hours; and in Mr. Goldson's case it reached to twenty-six hours; yet in none of these instances was difficulty experienced in seizing and bringing down the fetus out of the peritoneal cavity and through the vaginal laceration. Where the child has slipped into the belly, through a rupture exclusively confined to the uterine structure, the same facility in turning is not to be expected, because with the contraction of the womb the rupture will undergo a proportionate diminution in its size.

Baudelocque, with his accustomed shrewdness, has noticed this practical difference between uterine and vaginal lacerations; and certainly the point is one well deserving of attention.

If the child have been so many hours in the belly that its extraction *per vias naturales* is impossible, or possible only by doing great violence to the soft structures, then the easier and less dangerous course would be to remove the child by gastrotomy; or, even though no unreasonable length of time had elapsed since the recession of the fetus into the peritoneal cavity, yet if the patient had a contracted pelvis, and was come to the full time, I am convinced it would be giving her a better chance of recovery to withdraw the fetus by the abdominal section than to subject her to the tedious and troublesome business of delivering it according to

the ordinary method of turning. In giving this qualified sanction to gastrotomy I am justified by the numerous recoveries which have followed the operation when performed for rupture of the uterus; in fact a larger proportion of cures have resulted from this than from any other mode of delivery; and I cannot suppose it would be less successful after laceration of the vagina, though I am sure it would not be so often necessary.

Two successful cases of gastrotomy, after rupture of the uterus or vagina, have been recently published—one by Dr. Dyer (*British Medical Journal* for September 9, 1865), where the interval between the occurrence of the rupture and the extraction of the child was four hours and a-half; and in the other case, related by Dr. Crichton (*Edinburgh Medical Journal*, 1864), the interval was fourteen hours. In neither case was there any attempt made to withdraw the child by turning.^a

Dr. Murphy strongly advocates gastrotomy in all cases of rupture where the child has escaped into the belly; but I cannot altogether agree with him that “when the child is in the cavity of the abdomen, forced thither by the uterus or by the hand of the practitioner, the only operation that appears to give a reasonable chance of success is gastrotomy.” This opinion may hold good if there be pelvic deformity, or if the seat of rupture be the body or fundus of the uterus; but it is not tenable, I think, if the rupture be in the vagina or os uteri, and that the pelvic capacity is unabridged.

I have before remarked that the placenta sometimes follows the child through the vaginal rent into the general cavity of the abdomen. Where this has occurred its artificial extraction should be effected immediately after that of the child. In doing this the excellent rule of following the cord as our guide to the placenta should be implicitly attended to. On the other hand, if the placenta remain still in utero, we may ordinarily wait for ten or fifteen minutes, to allow time for the natural efforts to expel it. Should

* A deeply interesting question here suggests itself. The recoveries after gastrotomy (for ruptured uterus or vagina) greatly exceed the recoveries from Cesarean section, which is quite contrary to what we might, *à priori*, have supposed. How is this difference to be explained? The gastrotomy group of cases, as a whole, probably possessed somewhat better constitutions; but surely this is not sufficient. Herein, I think, lies the cause of difference—the situation and direction of the artificial opening in the genital canal. In the one category the opening is longitudinal, and is situated in the fundus and body of the uterus; in the other category it is circular or oblique, and occupies the vagina or neck of the womb.

it not then come away, however, and that gentle pressure on the uterus, with traction of the cord, fails to dislodge it, our best course, I believe, is at once to set about extracting it manually. It may be worth while remarking that I have never but once found it necessary, in any case of ruptured uterus or vagina, to pass the hand into the uterine cavity for the purpose of removing the after-birth; but I can readily understand that such an operation will require extreme care and gentleness in its performance, if we would avoid increasing the damage that the soft parts have already sustained.

The placenta having come away, we can now ascertain, if we have not already done so, the exact situation and extent of the laceration. At the same time we should make sure that no portion of gut has prolapsed through the rent. We should next endeavour to place the edges of the laceration as accurately in contact as can be done under the circumstances.

It can scarcely be doubted but that the permanent retaining of the torn parts in apposition would conduce to the chances of the patient's recovery; and, with the proper instruments, I really think there could not be any great difficulty (the vagina being so relaxed and capacious) in making two or three—not more—stitches with wire suture, just to hold the edges of the laceration together.^a

Of course, I fully concede that the circumstances under which this accident commonly occurs are such as to preclude the possibility of attempting this. But I need not remind you that our object should be to find out what is the utmost art can do to remedy this accident; and, knowing this, our endeavour should be to carry it out in practice as far as circumstances will admit.

^a Dr. Marion Sims relates (in his "Clinical Notes on Uterine Surgery") two cases which have a close bearing on the subject before us, as in each the peritoneal cavity was opened during operations upon the uterus. In one case the opening was through the cervix, and "would easily have admitted the passage of three fingers at a time into the peritoneal cavity" (p. 133). In the other case the chain of the *écraseur* made "an immense hole of a semilunar form, in the *cul-de-sac* of the vagina, through which we could look for three or four inches up into the peritoneal cavity, and observe the movements of the viscera with every respiratory act" (*op. cit.*, p. 207). Now, in each of these instances he closed the opening by bringing the edges together with metallic sutures, with as little delay as possible, and *both patients recovered*. In the former case he carefully removed, by means of sponge probangs, the blood which had forced its way into the cavity of the peritoneum, and closed the wound completely by five or six points of suture.—This woman "recovered rapidly." In cases of lacerated vagina or cervix I do not see why the same means should not be used to remove the blood which may have got into the serous cavity, and whose presence there must exercise a pernicious influence.

The soundness of the principle on which the above suggestion is based can hardly be disputed. Whenever a serous cavity is penetrated, whether accidentally or intentionally, is not the first step towards effecting a cure to carefully close the unnatural opening, and by union of the wound to permanently seal up the cavity again?

The passing of the sutures could not occupy many minutes, neither could this proceeding add to the existing dangers. Practically, the only difficulty I see in the way of carrying out the suggestion, supposing we have the necessary instruments at hand, is how to command a sufficiency of light, as of course the patient cannot be moved; but this difficulty is not one of an insuperable kind.

The exhibition of stimulants is usually needful during and immediately after delivery; nor can we safely suspend their use until decided symptoms of reaction begin to show themselves.

The subsequent treatment it is needless for me to describe, as it is essentially the same as that required in rupture of the uterus. The main points to be attended to, I think, are these, viz. :—

1. To enjoin absolute quietude of body. The urine should be drawn off with the catheter, to save the disturbance which the use of the bed-pan would cause.

2. The opium treatment should be fully carried out.

3. Nourishment, in the form of beef-tea, strong mutton-tea, or chicken-tea, should be given in small quantities, and at short intervals.

4. If the symptoms of peritoneal inflammation become developed, I would apply leeches over the seat of the greatest tenderness, in numbers proportionate to the patient's strength; and would employ mercury externally and, with due care, internally.

5. The utmost caution and reserve should be used with regard to purgative medicine. Some days should be allowed to pass before any attempt be made to procure an evacuation from the bowels.

Where a patient escapes the more immediate dangers arising from the nervous shock, and from peritoneal inflammation, there is, at a later period, yet another danger to which she is exposed, and that is pelvic abscess, behind or to either side of the uterus, and in connexion with the injured structures.^a I do not know whether

^a This danger seems to have entirely escaped the notice of systematic writers upon lacerations of the uterus and vagina. Nevertheless a careful perusal of the clinical records of this accident will serve to convince one of its existence.

this result ever follows ruptures of the uterus. All the examples I have found of it were cases of laceration of the vagina; and I can well suppose that it would not be at all so likely to supervene upon a rupture of the fundus or body of the uterus. In some, if not in most instances, we may attribute this abscess to the entrance of blood and air into the peritoneal sac; whereupon, as happens in certain exceptional cases of idiopathic pelvic hemocele, the cyst which forms around the effused blood takes on suppurative action. In the idiopathic or spontaneous pelvic hemocele suppuration is extremely rare; whereas in the traumatic pelvic hemocele, that, namely, which results from vaginal laceration, suppuration is not uncommon, and is to be accounted for partly by the presence of the rent in the vagina, and partly by the presence of atmospheric air along with the extravasated blood.

Pelvic abscess formed in three of Collins' cases of laceration of the vagina. One of these cases recovered and two died, and in each of these it was ascertained at the *necropsy* that the psoas muscle was more or less involved in the abscess. One patient died on the sixth and the other on the twenty-sixth day. When I was an assistant at the Lying-in Hospital I saw a patient who, on the forty-second day after a rupture of the vagina or cervix, died in consequence of an extensive abscess at the left side of the pelvis, extending upwards to nearly as high as the origin of the psoas muscle.

A recovery from laceration of the vagina and cervix occurred under my own care, at the Lying-in Hospital, in 1860, the details of which Dr. Byrne read to the society; and in this instance a very offensive puriform discharge was observed flowing from the vagina on the twelfth and succeeding days.^a

A case is related by Collins which clearly shows the possibility of internal secondary hemorrhage occurring. The laceration was situated "anteriorly, at the union of the uterus and vagina." The woman sank on the ninth day, and at the *autopsy* the immediate cause of her death was found to be hemorrhage into the abdominal cavity. A slight discharge of blood from the vagina had shown itself some hours before dissolution.

In my sixth case (already related) it may be remembered that on

^a I have not included this case amongst the examples of laceration of the vagina as the full extent of the tear was not ascertained, although it is certain the vagina was chiefly, if not exclusively, implicated.—See Dr. Byrne's report of the case in Vol. xxxiii. of this Journal.

the fourth day, after a movement of the bowels, produced by the enema, a considerable quantity of grumous blood was discharged from the vagina. This, I have no doubt, came from the peritoneal cavity, as a great deal of the like fluid was found in it after death. Most probably, however, all this blood had been extravasated a short time subsequently to the laceration. Although not exactly in point, still the clinical fact is worthy of note.

I shall now briefly recapitulate the principal points in respect to which laceration of the vagina or os uteri seems to differ, at all events in some degree, from rupture of the cervix or body of the uterus:—

1. Premonitory symptoms are very rare.
 2. The immediate constitutional effect of laceration of the vagina is not, on the whole, so profound as that arising from rupture of the uterus.
 3. Vomiting is occasionally a symptom of the accident, but it is not of the *coffee-grounds* character.
 4. The laceration is very rarely, if ever, induced by deformity of the pelvic brim.
 5. The head is commonly engaged in the pelvis at the time the laceration occurs.
 6. The tear can in no way be attributed to contractions of the structure directly involved.
 7. The laceration almost always takes a circular direction, and
 8. Remains patulous, or at least shows very little disposition to contract.
 9. The escape of the fetus into the peritoneal cavity follows more frequently upon vaginal laceration than upon uterine rupture.
 10. The escape of the placenta, likewise, through the laceration is more apt to take place here than in ruptures of the uterus.
 11. Prolapse of the intestine, also, is a less rare complication of vaginal laceration than of uterine rupture.
 12. The operation of turning is found to be practicable for a longer period after laceration of the vagina than of the uterus.
- The four preceding characteristics (Nos. 9, 10, 11, 12) naturally result from the peculiarity stated in No. 8, which belongs to lacerations of the vagina or os uteri.

13. There is a greater liability to pelvic abscess after vaginal laceration; and this we may, in some degree, attribute to the greater likelihood of atmospheric air entering the belly through the solution of continuity in the vaginal canal.

14. Lastly, a comparison of the mortality of these two lesions clearly proves that vaginal laceration is a much less fatal accident than uterine rupture.^a

ART. XII.—*Notes in Medicine and Surgery.*—V. By PHILIP CRAMPTON SMYLY, M.D., Univ., Dub.; F.R.C.S.I.; L.C.P.I.; Surgeon to the Meath Hospital, and to the Institution for Sick Children, Pitt-street.

ASCARIS LUMBRICOIDES IN THE TRACHEA.

PATRICK F., aged three and a-half, a fine healthy-looking boy, was admitted into the Meath Hospital on Friday, 9th February, 1866, on account of great difficulty of breathing.

The child's sister, who brought him to the hospital, gave the following account:—He had been in perfect health until dinner time, about four o'clock. She left the room for a few minutes, when on her return some one exclaimed "the child is choking!" She thought a piece of cake the child had been eating had stuck in the throat. Failing to get it down she at once brought him to a doctor some three miles off. He tried to push down the supposed obstruction with an instrument, but failing to relieve the breathing, he sent him to hospital.

Mr. Owens, on admission, endeavoured to remove the obstruction by introducing his finger two or three times, and he also used a probang. The symptoms of suffocation becoming every moment more intense he sent for me.

The child was in a state of complete anesthesia; the eyes were half closed, and beginning to glaze; the respirations were very laboured; he was apparently dying from some obstruction at the top of the windpipe. From the hurried history I received I concluded that the child had taken boiling water. The girl had not been in the room when the child began to choke. The symptoms had become rapidly and steadily aggravated; while the finger had failed to detect, and the probang to remove the supposed cause of the obstruction.

^a At the Meeting of the Obstetrical Society following that at which the above paper was read, Dr. J. A. Byrne communicated the particulars of a case of extensive laceration of the vagina, ending in recovery. It very well exemplified all the leading points above laid down as being characteristic of vaginal laceration. The history of this important case will doubtless be published.

I passed my finger, with some difficulty, between the teeth into the pharynx, when it met a soft spongy mass. On pressing through this the finger distinctly entered the larynx. I concluded that this soft mass was œdema, the steady increase of which had given rise to the steady increase of the symptoms.

I proceeded to prepare the instruments for laryngotomy, but on returning to the ward I found that the child had ceased to breathe for some minutes; there was no pulse at the wrist; the face had become livid, and the eyes were quite glazed. Having among my instruments a catheter open at both ends, I passed it at once into the larynx, guided by my first finger, and pushed it on into the trachea; but without effect. Artificial respiration was then performed; I filled the chest by blowing through the catheter, and emptied it again by pressing with one hand at either side over the false ribs. This process was continued for more than twenty minutes before the child gave a gasp, and shortly after Professor Macnamara, who held the wrist, reported that he felt the pulse returning. After three quarters of an hour respiration was fully established through the catheter. The pulse became full at both wrists, and the respiratory murmur audible over the chest.

A consultation was then held whether laryngotomy should be performed or the catheter be left in the trachea.

The operation was decided on, and was performed at once. The breathing became much embarrassed when the catheter was removed, though the air passed freely through the tracheal tube. Shortly after the operation the child seemed to be sinking; some brandy and water by the mouth, and some brandy and turpentine by the rectum revived it again. For some hours it breathed calmly through the tube. Convulsions, however, set in during the night, the attacks increasing in violence and frequency until about eight o'clock on Saturday morning. The intervals then became longer, and the attacks less violent until four o'clock on Sunday morning. The pulse then began to fail, and the child died two hours afterwards.

Respiration was performed entirely through the tracheal tube until Saturday mid-day, when air was expired through the mouth and nose as well as through the tube. The child swallowed beef-tea and wine without difficulty during the thirty-six hours he was in hospital.

On Monday morning the *post mortem* was performed by Mr. Owens, the resident. When the tongue was drawn down, after detaching the structures from the lower jaw, the pharynx was found filled with a mass of round worms. The parts were then

removed together; and on laying open the œsophagus, so as to see the top of the windpipe; no œdema was discovered, only slight traces of inflammation about the epiglottis and arytenoids. Protruding, however, from the glottis was the head of a round worm. See Fig. 1. The preparation was then turned round, and the trachea divided from the opening made at the time of the operation down through the bronchial tubes. This section brought to view the rest of the worm. It lay with its head protruded from the larynx, the tail just below the vocal cords, and the lap resting on the bifurcation. See Fig 2. Both drawings were made by Mr. Burnside, Mr. Lewis' talented artist, from the preparation mounted by my friend Dr. Barker for the Museum of the Royal College of Surgeons.

There were no worms in any other part of the body though all the organs were examined.

“Royal College of Surgeons, Ireland,
“Feb. 13, 1866.

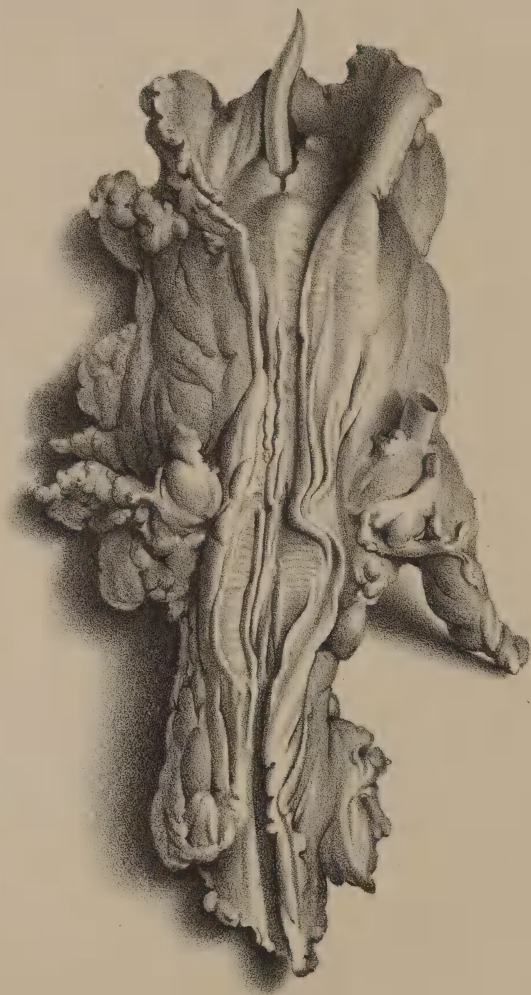
“MY DEAR SIR,—The preparation you brought to the college yesterday is remarkable for the number of entozoa, the position in which they were found, and the fatal result. There were six specimens of the *ascaris lumbricoides*—two of them males. Yours most truly,

“JOHN BARKER.”

The operation of tubing the larynx in cases of obstruction at the top of the windpipe had been suggested to me about two years ago by my friend Dr. O'Flaherty. He had not himself performed the operation, but pointed out the advantages of the proceeding in such cases. I determined to try it whenever a suitable case occurred, and accordingly placed one of Hutton's catheters with my instruments for tracheotomy.

Since the operation I have found that M. Bouchut has recommended “tubing the glottis as a substitute for tracheotomy in croup, and leaving for a time in the orifice a metallic ring.” In Art. 5551 of the *Journal of Practical Med. and Surg.*, 1st year, English edition, the editor concludes in the following words:—“All that can be said—and M. Bouchut has kindly permitted us to witness the operation—is that the tubing the larynx is not a difficult process, that the canula remaining in the glottis for thirty-six hours was perfectly harmless; that the two children could speak distinctly, and take liquids without swallowing them the wrong

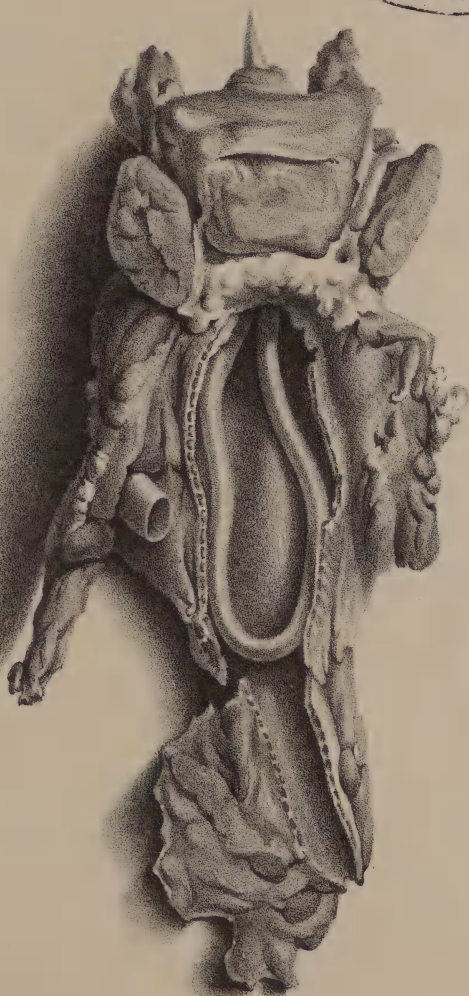
PLATE I.



Dr. P. C. SMYLY

Ascaris Lumbricoides, head protruding from the Larynx,
Esophagus laid open

PLATE II.



DR. P. C. SMYLY.

Ascaris Lumbricoides in the Trachea.

way; and that there was in every respect a temporary improvement analogous to that which follows tracheotomy."

It is very rare to find a lumbricus in the trachea, though worms are not unfrequently found in the mouth after death, and sometimes during life.

After death worms almost always leave the body. A gentleman lately returned from Penang told me that in Malay, where murders are frequently committed by cutting the throat, the horror of the scene is greatly increased by numbers of round worms leaving the body through the gash, and wriggling and writhing in the blood.

In fevers and other diseases worms leave the body, and "after long periods of abstinence or inanition they have been found to migrate and escape from the anus, mouth, and nose."^a

Sometimes they leave the body without any apparent cause. My friend Dr. Robert White has in his possession a number of round worms which a woman removed from the mouth of a child otherwise in perfect health.

In most of the general treatises on medicine the fact is stated that worms have been found in the larynx and trachea.

Copland says:—"When the worm passes into the stomach, it may rise rapidly into the upper part of the œsophagus, or into the pharynx, and even get entangled in the larynx, and produce the most distressing effects. But such occurrences, although observed, are very rare."^b

Neimeyer says:—"Der spulwurm wohnt im Dünn—und Dickdarm, macht aber von dort aus weite wanderungen und kann dabei in den magen, in den œsophagus, selbst in den larynx gelangen."

The only parallel case I can find is recorded in Andral's *Path. Anat.*, p. 210:—

"These worms sometimes quit the alimentary canal, and make their way into different neighbouring parts. M. Blandin and I once found, at the *Hôpital des Enfants*, an ascaris in the cavity of the larynx; it occupied the space between the cordæ vocales, and part of the body was still in the pharynx. The child had been suddenly attacked with extreme dyspnea, and died very shortly after in a state of asphyxia."

Dr. E. P. Wright very kindly wrote me the following note:—

^a Copland's Dic., p. 1,405.

^b Copland's Dic., p. 1,405.

“DEAR SMYLY,—I have hunted up what has been written on the subject of nematoid worms and can only find one record of a case at all like yours, viz.:—J. H. Cutler—death by worms—a large lumbricus being found in the windpipe.”—*Boston Med. and Surg. Journal*, Vol. LXVI., p. 392, 1862.

Dr. Robert Smith and Mr. William Stokes have kindly called my attention to a case reported in the *Trans. of the Dub. Path. Soc.*:—

“Dr. Law presented a specimen of the ascaris lumbricoides, about a foot in length, and apparently full of ova, passed by the mouth, by a female, aged 22 years, while labouring under typhus fever. In the evening of that day she had a violent attack of coughing: the nurse, supposing that she was choking from a collection of mucus in the mouth and throat, came to her relief, and drew out of her mouth the worm now exhibited. It can easily be imagined that the specimen now exhibited was either *en route* towards or had already made some way into the trachea, from which it was expelled by coughing.”

There is no mention of this accident in any of the more recent works that I have been able to consult.

ART. XIII.—*Cerebro-Spinal Meningitis.* By ROBERT LAW, M.D.,
Professor of Institutes of Medicine in School of Physic.

EARLY in the last year we were alarmed by the accounts of a disease which had appeared in different localities through Europe, but chiefly in Russia and Prussia. This disease was variously described. It was supposed to have some of the characters even of the plague. It was thought to be one of the various forms of low asthenic fever with which we have been so familiar in latter years, many of which, although not exactly similar, had such a strong family likeness that it might be said—*facies non omnibus una, haud diversa tamen*. The fever was said to resemble, in many particulars, that which prevailed in this country after the year of scarceness, and which got the designation of famine fever. Again, another form that this disease assumed was that of cerebro-spinal arachnitis, a formidable disease, of which we had a few instances,

but these of a very severe and fatal character. The Editor of the *Dublin Medical Journal*, sensible of the benefit that had accrued to medical science from the valuable reports of Barker and Cheyne on the fever that had prevailed in this country in the years 1817, '18, '19, when the famine fever was on the decline, invited the medical men who had had opportunities of witnessing the character and treatment of the disease to furnish him with the results of their experience, in order to publish them in the *Journal*, that they might become a permanent record. Amongst the communications supplied to the *Journal*, I contributed my contingent, which is in No. XVI., Nov., 1849. In my observations, I remarked:—"The most common and loudest complaint of our patients was an aggravation of what the subject of fever generally describes as pains in the bones, but which really means pains in the course of the spinal nerves. While in former fevers this complaint was seldom more than that of a contused or bruised feel, or of such a sense of discomfort or fidgety restlessness as the French so significantly express by the term *malaise*, here the individual in many cases seemed to suffer as intensely as in the severest cases of acute rheumatism. These pains were more or less general in different patients. In some they affected the back of the head and neck; in some they only ran down the legs; while in others they spread themselves through the whole body, and embracing the sides, imparted the sensation of painful constriction. The nape of the neck and across the loins were the points to which the patients most frequently referred their pain. When it affected the former position, which it did in many cases, we regarded it with some degree of apprehension, from this being the starting-point of that formidable disease, cerebro-spinal meningitis, into which we are sure it would have passed had it not been arrested by treatment." The reason for our transcribing these observations is that they exactly describe cases which presented themselves to us in the course of the last Summer, and which seem to us to deserve the designation of cerebro-spinal meningitis. Although the majority of the cases that came under our notice were females, and with these assumed the features of aggravated spinal irritation, the two first cases were young men. The first was a young man, aged twenty-one, who had been in a grocer's establishment. He was admitted into the hospital. All his members seemed to be paralysed. He could move neither arms nor legs. He had a vacant stare, pupils dilated, nor could he understand anything that was said to him. There was no abnormal heat of skin. He twisted

his body, and had a general fidgetiness, so that he required to have a person constantly beside his bed to prevent his wriggling himself out of it. Pulse not frequent, but small and weak. Although the weather was unusually warm, his hands had the appearance of being affected with chilblains. The legs exhibited a mottled congested appearance. We could obtain no satisfactory account of the patient's antecedents, either before his illness or from its commencement, to the time of his admission into hospital.

We directed leeches to be applied behind the ears and along the spine, turpentine stupes to the legs, and to have the following mixture:—Sesqui-carbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; camphor-water to eight ounces. An ounce to be taken thrice daily.

He now became very noisy and excited, appearing to awaken, as it were, from a stupor. We then ordered for him the following mixture:—Tartar emetic, one grain; Battley's sedative liquor, thirty-five minims; spirit of nitrous ether, half an ounce; camphor-water to eight ounces. An ounce to be taken thrice daily.

He took but two doses of this mixture when he became quite calm; he was no longer noisy, but exhibited a stupid, vacant expression. He was extremely weak, and had hardly any use of either his arms or legs. We allowed him wine liberally, and hot tea, and had a stimulating liniment, rubbed along the spine, and turpentine stupes to the legs. We afterwards ordered him the following mixture:—Sesqui-carbonate of ammonia, one drachm; Hoffman's sedative liquor, half an ounce; infusion of arnica, three ounces; camphor-water, to eight ounces.

He improved but very slowly, and ultimately got quite well; and when well he had so different an appearance from what he had when he first came under our care that we should have questioned his identity. He was about a month in hospital.

The next case was that of John Keogh, aged nineteen, who was equally helpless as to the use of either legs or arms, and had to be carried into the hospital. He was literally in torture from pain in his temples, forehead, and spine, and extending into legs and arms. The pulse was not abnormally quick, nor was the skin unusually hot, but his exhaustion seemed to be extreme. We directed a blister, sprinkled with two grains of morphine, to be applied to the forehead, and leeches along the spine. The blisters to the forehead brought very little relief, but the leeches nearly removed the pain

from the parts to which they were applied. We now directed leeches to the temples, and they were as successful in removing the pain in forehead and temples as were the others in removing those of the spine. It was most striking the change of expression that ensued almost immediately after the leeches had been applied; the countenance lost the look of anguish that it had before. We ordered an anodyne liniment, composed of camphor-liniment, chloroform-liniment, and tincture of opium, in equal parts, to be rubbed along the spine, and directed the following mixture:—Sesquicarbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; infusion of arnica, three ounces; camphor-water to eight ounces. An ounce to be taken thrice daily.

His pains soon entirely disappeared, but he was very long recovering the use of his limbs. His debility was very great. He was a month in hospital before he recovered his strength.

Margaret Kennedy, aged sixteen, admitted into hospital June the 16th, 1865. She had been five days ill before her admission into hospital. Her illness began with shivering and pains all through her. She now complained of her back and of most distressing pains through her arms, and legs, and sides, as if she were tightly bound round with cords. She experiences great pain when the spine is pressed even slightly in the lumbar region. She feels extremely weak and exhausted. Stomach very irritable. Tongue loaded. Abdomen tympanitic. She was ordered cold lotion to her forehead and leeches to the portion of the spine that was painful when pressed, and the following mixture, with the view to quiet the irritability of the stomach:—Acetate of lead, ten grains; acetate of morphia, one grain; vinegar, two drachms; syrup, six drachms; water to eight ounces. An ounce to be taken thrice daily.

She was ordered four ounces of brandy. Our report was:—Head relieved. Sickness of stomach ceased. Much less pain in the back and through the limbs. We directed the following liniment:—Compound camphor liniment, two ounces; aconite liniment, tincture of opium, of each an ounce. This liniment to be rubbed along the spine.

She slightly improved from day to day, the pains in the limbs becoming less, but when they became less in one part they invaded another. The mammæ, which were unusually developed in so young a person, became their seat in a very aggravated degree, but

were relieved by stupes. The tenderness on pressure of the spine continuing, a belladonna plaster afforded some relief. Although the pains of the limbs lost a good deal of their severity, they were still very distressing.

She complained much of oppression of the chest, and had a good deal of bronchitis.

She had a very tedious convalescence. Her weakness was extreme. She was very long before she could use her limbs.

Ellen Holohan, aged twenty-two, unmarried, admitted into hospital June 30, complaining of severe pain in temples and forehead, and root of the nose; also in back and in arms and legs. She had no fever, but great weakness and loss of appetite. She stated that she had been three weeks ill, and that her illness began with shivering and a feeling of cold water trickling down her back. She then became hot, and then had perspirations, and then had pains in her back. This set of symptoms did not recur, but she was extremely weak; yet she continued up and about, until, as she said, every member but her tongue became paralysed. Eight leeches were directed to be applied along the spine, cold lotion to the forehead, warm stupes to the legs, and to have the following mixture:—Sesqui-carbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; camphor-water to eight ounces. An ounce to be taken thrice daily.

July 2nd—Pains relieved.

3rd—Has a good deal of pain in the forehead and root of nose, and in the nape of neck; pains in back and legs much lessened. Four leeches to be applied to the nape of neck. Other things to be continued.

5th—Pain in back of neck gone, those of back and legs much relieved. Liniment to back, and stupes to the legs to be continued. She continued to improve, and no note was taken till the 9th, when she complained of distressing acidity of stomach and of a strong pulsation in the epigastrium, for which we directed the following mixture:—Liquor of potash, a drachm; compound tincture of gentian, two drachms; syrup of orange-peel, five drachms; infusion of colombo to six ounces. An ounce to be taken thrice daily.

She now gradually but very slowly recovered her strength, and left the hospital on the 21st.

One of the most remarkable features of this case was the striking

improvement in the expression of the countenance that took place as her symptoms gave way. I attach importance also to the derangement of the stomach, indicated by acidity, as I had occasion to observe the same symptom to occur to other persons at the same time, while they only complained of this with a sense of unusual sickness and exhaustion. At the time Holohan was admitted into the hospital I had there under my care her two brothers and two sisters, all affected with fever. The fever under which the two sisters and one brother laboured was comparatively mild, but that of the elder brother, aged seventeen, although mild at first, assumed a very alarming character in its progress, exhibiting symptoms of high cerebral excitement, which passed into profound coma, from which, happily, he was roused by a large blister applied to the vertex. As the blister rose he seemed as it were to awaken out of a deep sleep. The skin in this youth's case was bathed in perspiration, and afterwards thickly covered with sudamina. I would here incidentally allude to the great advantage derived from the application of a blister to the vertex in cases of either actual or threatening coma supervening on high cerebral excitement. We have observed in many cases, as in the one just recorded, how, when the blister began to rise, the patient seemed to awaken, as it were, from a deep sleep.

At the same time that Holohan and Kennedy were in hospital, there were three sisters—Bridget Fannin, aged twenty-two; Ellen Fannin, aged twenty; and Margaret Fannin, aged nineteen—also under my care in hospital, exhibiting somewhat similar symptoms. The eldest and youngest were admitted on the 3rd of July, supposed to be cases of fever. When they were visited the day after their admission they did not exhibit symptoms of fever, the younger especially only complained of not being well, but could hardly say what was the matter with her. She only remained with us three days, exhibiting from the first an impatience of being in hospital, which we learned afterwards was owing to a matrimonial speculation which she feared her illness would interfere with. The elder sister's illness was of a much more serious character. She complained of most distressing pain in her temples, her forehead, and all through her head, also in her back, along the spine, which was especially sensitive to even slight pressure, and of pains in both legs and arms. Her countenance bespoke the most intense suffering. The skin was not abnormally hot. The pulse was above 110 in the minute. Her prostration

was extreme. Two leeches were applied to each temple, and eight along the spine. The legs were stuped. She was ordered six ounces of wine. These means afforded some relief, but her pains both in head, and back, and legs were still very distressing. In a few days twelve more leeches were applied along the back, and the stupes to the limbs, which soothed her very much. The following liniment also was rubbed along the spine, in the interspaces between the leech-bites:—Compound camphor liniment, three ounces; tincture of aconite and chloroform, of each half an ounce

The leeches always brought relief. She still complained of pain in the forehead, for which a blister was applied there. Her extreme prostration made us increase the quantity of wine, and also to allow her some brandy. The blister relieved the pain of the forehead, but the pain in the back and legs still continuing, twelve more leeches were applied, and the stupes continued. On the 14th we pronounced her convalescent, her pains being gone, but she was extremely weak, and felt as if she had not a leg to stand on. She was very deaf. We directed for her the following mixture:—Sesqui-carbonate of ammonia, one drachm; infusion of arnica, two ounces; tincture of orange-peel, half an ounce; decoction of bark to eight ounces. An ounce to be taken thrice daily.

Her convalescence was very tedious. She was very slow in recovering her strength. Her stay in the hospital was altogether a month.

On the 8th of July the youngest sister, who had been under our care for fever, but left us after three days, was readmitted, accompanied by her second sister, Ellen Fannin. The former complained of most distressing pains in her legs, and of extreme sickness. Her countenance expressed deep suffering. Her appearance in this respect differed much from what it was before when she was with us. The second sister, Ellen, complained of most distressing pains at the lower part of the back and in hypogastrium, which was most sensitive to the slightest pressure—nay, rather to the touch. Pressure was not only more tolerable, but even afforded relief. There was considerable tympanitic distention of the abdomen, and a tumour could be felt above the pubes. From the coincident symptoms we regarded this tumour to be what is designated a phantom tumour, and were sure that it would disappear with the other symptoms. She complained of the same distressing weakness and exhaustion as her sister. For her we directed the following

mixture:—Spirit of chloroform, two drachms; camphor-water to six ounces. An ounce to be taken thrice daily.

Our reason for this prescription was our experience of its value in hysterical tympanitis. We also directed the abdomen to be fomented with camomile and poppy-heads, and that she should have four ounces of wine. For her sister, who only complained of pain in her legs, we only directed the legs to be stuped, and that she should have four ounces of wine. The stupes always relieved her pains for the time, but they continued to return for some days, and finally ceased. The means employed with Ellen Fannin relieved the pain of back and hypogastrium, but there was still considerable tympanitic distention of the abdomen, for which we directed turpentine stupes, and the following pills:—Compound galbanum pill, twelve grains; sulphate of quinine, four grains; extract of *nux vomica*, two grains; to be made into four pills. One to be taken thrice daily.

My reason for making quinine an ingredient in my prescription was that I regarded the disease to be a kind of malarious fever, especially affecting the spinal nerves, but not confining its influence to these, but involving the brain and also the ganglionic nerves. The tympanitis was somewhat relieved, but she now was affected with most distressing irritability of stomach. The only thing it retained was wine; everything else it rejected. We directed for her effervescing draughts, with laudanum. They did not succeed; nor did a blister, sprinkled with two grains of acetate of morphia, applied to the epigastrium. We then directed the following mixture:—Liquor of potash, one drachm; dilute hydrocyanic acid, twelve minims; liquor of hydrochlorate of morphia, half a drachm; tincture of orange-peel, three drachms; infusion of mint to eight ounces. An ounce to be taken thrice daily.

Her stomach now became settled, but she was extremely weak from this organ so long rejecting food in every shape. We had ordered her enemata of beef tea and brandy—an expedient to which we had resorted with great success in cases where a weak stomach could not do as much as the failing strength of the patient required. We have a vivid recollection of one interesting case where the pulse had ceased and the power of deglutition was gone, and when from such enemata ebbing life seemed to return. Nor have these instances been few. In the present case the stomach soon was able to take a due supply of food, so that we had no occasion to resort to this method. Her pains had now ceased, and we had now only to deal with the

weakness, which was extreme. The youngest sister, who only complained of pains in her legs, and which yielded to continued stupor, exhibited the same prostration, and also then became quite deaf. They all felt, as it were, a sense of overwhelming lassitude and fatigue amounting almost to that of paralysis.

They were extremely slow in recovering their appetites, and could only take wine or brandy; while the illness of the eldest, which was much more severe than that of the other two, and required more active treatment, or to which attack more active treatment was applied, as she had thirty leeches to her head and back, lasted a month; theirs lasted three weeks: still the cure of the eldest appeared to be more complete, and she appeared to recover her strength sooner than her sisters.

I learned that the father of these young women had died about a month before their illness; and the account that I got of his illness was that he had a kind of fever which ended fatally in an attack of apoplexy, attended with hemiplegia. He had been a man of very intemperate habits. We think we can discover a kindred relation between the illness of the parent and the children, the same morbid influence acting on both, but modified in its action on the parent by his previous habits.

Anne Walsh, aged forty, married, admitted into hospital July 21, complaining of most distressing pains in the forehead, temples, down her back and through her arms, down to the very tips of her fingers, and down her legs, even to her toes. She said she felt as if her feet were asleep. She described her sense of sickness and prostration as extreme, which her expression seemed fully to confirm. We directed leeches to be applied along the spine; a blister, sprinkled with two grains of sulphate of quinine, to the forehead, and that she should have two grains of quinine made into a bolus with aromatic confection, three times daily; and an ounce of the following mixture after each bolus:—Sesqui-carbonate of ammonia, one drachm; Hoffman's anodyne liquor, two drachms; camphor-water, eight ounces.

22nd. Pains of limbs much relieved, but pain of head as distressing as ever, both front and back; two leeches were directed to be applied to each temple, and six to the nape of the neck.

23rd. Head is entirely free from pain—in fact, all her pains have left her. Her extreme prostration, and that she got no sleep at night, were now her only complaint; for the latter we directed a hypnotic

draught of five-and-twenty drops of Battley's sedative, and for the former continued the boluses of sulphate of quinine, with the mixture of ammonia, Hoffman's anodyne, and camphor-water. We also gave her a liberal allowance of wine. She recovered her rest, and gradually improved in strength, so that on the 1st of August we pronounced her well, she then only complaining of noise in her ears—a symptom of which almost all patients affected like her complained, some more and some less.

Jane Nugent, aged twenty-eight, admitted into hospital July 19, complaining of pain in back and stomach. Has no headache. Skin hot, and densely spotted with a bright, florid efflorescence. Pulse 120—soft. Tongue red at point and edges. Complains of great prostration. Feels considerable pain when spine is pressed.

Ordered bolus of three grains of sulphate of quinine, one grain of antimonial powder, and of aromatic confection, q. s., thrice daily; and the following mixture:—Sesqui-carbonate of ammonia, a drachm; Hoffman's anodyne liquor, two drachms; camphor-water to eight ounces—an ounce to be taken thrice daily. And the following liniment to be rubbed along the spine:—Compound camphor liniment, three ounces; chloroform and tincture of aconite, of each half an ounce.

20th. Experienced great relief from having her back rubbed with the liniment.

23rd. Abdomen very thickly spotted with bright, measly efflorescence, rather tympanitic, with tenderness on pressing the ilio-cecal region. Diarrhea, with yellow discharges. To have five grains of aromatic chalk powder with opium, thrice daily.

24th. Diarrhea has almost ceased. No tenderness on pressing the abdomen. Pain of the back not quite gone. Liniment to be continued.

She now had some delirium, which we attributed to the sudden ceasing of the diarrhea. Having frequently observed in typhoid fever cerebral disturbances to supervene on the diarrhea being checked, we are always on our guard, and take precautions against it by cold lotion to the forehead, and fomentations to the legs. These means succeeded in the present instance. She now went on favourably, and on the 31st we pronounced her convalescent, although extremely weak.

This was the only case of this group in which there was what could be called true typhoid fever. We made the experiment of

treating it with quinine and antimonial powder, regarding it as a kind of malarious fever, but when the diarrhea came on we did not feel ourselves justified in pushing this treatment further.

Master Ormsby, aged twelve, had been ill with sore throat and swelling of the glands of the neck, and severe pains in his limbs, with extreme prostration of strength. He felt utterly knocked down, while he had very little fever. We prescribed for him some cordial diaphoretic medicine. He very gradually recovered his strength, and the muscles of the neck continued stiff for some considerable time.

The mother of this boy was seized with pain in the top of the head and down her back, and in both legs and arms. She also had a stiff neck, and the muscles were very rigid. She completely lost her appetite, and was utterly prostrate. Her chief pain was at the nape of the neck. She attributed her illness to exposure to the night air. Leeches applied to the neck gave her great relief. She took no food. Brandy and milk was the only nourishment she had the least relish for. Her countenance betrayed her illness. She looked most wretchedly. There was no fever. She had most distressing noise in the ears. She took quinine, and had a liniment of camphorated oil and chloroform rubbed along the spine. For a considerable time she could take no food, and felt, as she said, utterly good for nothing. She could not take interest in anything. This state of things continued for a month, when, at last, she began to improve, and this improvement showed itself in her improved expression of countenance, which seemed, as it were, gradually to expand and to lose the pinched appearance that it hitherto had. She described, too, a thrilling sensation through her, which, she said, recalled what she felt when on the occasion of having a tooth extracted, she had inhaled chloroform, and was just recovering from its influence. She had pretty well recovered, and thought she need no longer spare herself, but a return of the stiffness of her neck told a different story.

We consider it a fact worthy of recording that at the time we were attending this lady, affected as we have above described, amongst rabbits which her sons had three died, each a month old, and each in the same way; its limbs seemed to fail it, and it fell on its side, then worked in convulsions, and died. Two hens also fell lifeless from their roost. I begged of her to let me have the first rabbit that died that I might have it examined, in order, if

possible, to ascertain the cause of death. One soon died exactly in the same way that the others did; it seemed to fall down, as it were, paralysed, then worked in convulsions, and so expired. I sent it to Dr. Connor, Curator of the University Museum, and begged him to examine it for me, not communicating to him any particulars relative to the animal's death; the only unusual appearance he observed was a very remarkable congestion of the vessels at the base of the brain. I sent him two more that had died exactly in the same way. In one there was the same congestion of the vessels at the base of the brain, and in the other there was a vascularity of the membranes of the spinal marrow, indicating inflammation.

Out of eleven rabbits nine died exactly in the same way.

At the time of the occurrence of these cases, a very remarkable case of tetanus occurred in a young lady in Dublin, and excited a good deal of interest. The cause assigned was that she had fallen asleep opposite an open window. There was no wound that could account for it. The disease terminated fatally in a week. I did not learn more exact details of the case, but what I did hear suggested to me the probability that its pathological character was the same as that of those cases that had come under my observation, but in a more aggravated form; and what especially made me entertain this idea was that many of the individuals under my care complained of stiffness of the jaws almost amounting to trismus.

What, we would ask, is the pathology of these cases? Or am I correct in the designation I have applied to them? Would the term *cerebro-myelitis* have been more strictly applicable? Two views suggest themselves to us. Either the morbid impression may have been first made upon the heart, and have weakened it, and in consequence of this weakened state of the organ, its propelling power was lost to the capillaries of the spine, and they thus became congested; and no doubt the effects of the remedies employed would give some countenance to this view; for the immediate relief that followed the application of leeches to the spine was most striking, and the beneficial effects of tonics and stimulants were equally remarkable. The other view is, of course, that the morbid impression was first made on the nervous system, and exercised a debilitating effect on all the parts under the influence of this system, that it was some malarious influence acting on the system. This view would seem to afford a more complete and satisfactory explanation of the phenomena that presented themselves,

viz., the pains, and the helplessness and loss of power of motion, and the overwhelming feeling of fatigue and lassitude of which most of the individuals complained. Nor did the nerves supplying the muscles, the instruments of motion, appear to be the only ones affected, but the ganglionic system of nerves also.

We should be disposed to adopt the latter view, while we confess how difficult it is to assign its proper place to a morbid complication involving the nervous and vascular systems—these two being so intimately connected and so constantly implicating each other. While we profess our preference for the latter view, we should say to others—*Utrum horum mavis, accipe.*

ART. XIV.—*Syphilitic Paralysis and its Treatment.* By WILLIAM MOORE, M.D., Dub.; M.R.I.A.; Fellow of the King and Queen's College of Physicians; Senior Physician to Mercer's Hospital; Physician in Ordinary to Sir P. Dun's Hospital, and to the Institution for Diseases of Children; Lecturer on Practice of Medicine, and on Clinical Medicine, &c.

Syphilis, Followed by Secondary Symptoms—Paraplegia—Hemiplegia—Epileptiform Convulsions, with and without Loss of Consciousness—Progressive Motor Ataxy.

The subject of nervous disorder supervening on syphilis has been treated of by various authors, among whom may be mentioned Drs. Graves, Todd, Brown-Séquard, Reade, Duncan, Hutchinson, Bryant, and others; but as these affections are daily receiving more attention the details of the following cases may not prove wholly uninteresting:—

CASE I.—*Syphilis, Followed by Sore Throat, Rheumatism, Cervico-dorsal Neuralgia, Paraplegia.*—Michael R., aged twenty-six, was admitted into Mercer's Hospital on the 6th of October, 1864.

His family history is as follows:—He had four sisters and one brother, who all died young; his mother is also dead, but his father is alive, and is a strong, healthy man.

This patient had been a waiter for the last twelve years, during which time he was very much addicted to drink, kept late hours, and was exposed to cold. About six years ago he contracted

syphilis, for which he took mercury freely; he afterwards had sore throat and syphilitic rheumatism—in short, from his first being infected his health has never been good. About three months before his admission into Mercer's Hospital he was seized with a severe pain in the nape of the neck and between his shoulder blades; for this he underwent some treatment which gave him temporary relief, but the pain returned, accompanied with shiverings. He now took Turkish baths, but obtained no relief. On the night of the 27th September he was seized with severe cramp in his right leg, and on the following day was unable to move it, and a sense of numbness extended over both legs.

On admission into hospital his appearance was anemic, and he complained of pain and weakness in the lumbar region, also of a pain and sense of tightness round the epigastrium. He had great difficulty in passing urine (which, however, had an acid reaction), and he was unable to turn himself in bed without being assisted. His appetite was fair, and he slept well.

He got five-grains doses of iodide of potassium, with one-sixteenth of a grain of strychnine, three times a day; electricity was applied along the course of the spine; at the end of a month he was able to run about the ward; and after ten days more he left the hospital, feeling able, as he said himself, "to attend a table of twenty."

CASE II.—*Syphilis, Followed by Secondary Symptoms—Iritis, Epileptiform Convulsions, with Loss of Consciousness, Hemiplegia.*^a—John D., aged forty, but looking ten years older, was admitted into Mercer's Hospital on the 7th November last. His history is as follows:—By occupation a gardener; he was a good deal exposed to wet; but in addition he had been in the habit of wading up to the waist when on fishing excursions. In 1860 he contracted syphilis, for which he took mercury freely, and at the same time his habits were intemperate; in 1861 he had syphilitic iritis, when he again got mercury. Two years ago he had an epileptiform attack, and remained unconscious for forty-eight hours; and within the last year an eruption appeared on his arms, forearms, thighs, and legs, which, judging from the cicatrices, I should say was rupia. On the night of the 3rd of May last he went to bed in his usual health, but on trying to dress himself the following morning he was seized with a loud buzzing in his ears, and fell down unconscious. From this

^a This case was reported by Mr. John Myles.

date he tried to follow his occupation till the week before his admission into the hospital, when he had another fit, on recovering from which the right side was found paralysed. None of his family were paralytic.

On examination there was complete hemiplegia of the right side of the body; there was blunting, but no absolute loss of sensibility. He had imperfection of speech, so much so as to be with difficulty understood; there was double vision and abnormal contraction of the pupils; his pulse was slow, labouring, and compressible; the urine was alkaline, and there was an involuntary passing of the dejections. On the 7th of November a blister was applied over the head from ear to ear, and he got six ounces of wine. The following day he seemed more himself, but still passed everything under him. He was now ordered five-grain doses of iodide of potassium, with syp. of bark.

On the 14th of November he was still passing involuntarily; the urine was alkaline, but he spoke more distinctly and had more command of himself generally. The left pupil was dilating, but the right remained minutely contracted. On the 20th the treatment was changed to bromide of potassium, seven grains three times a day. During the night of the 22nd he again had the "buzzing" noise in his head and pain in the cervical region, and on the 23rd he had an aborted epileptiform attack, with unconsciousness. On the 24th he was much more lively than he had been since his admission, and gave timely warning to the nurse: the bromide of potassium was increased to ten grains three times a day. On the 7th of December we went back to the iodide of potassium. He was now able to walk about the ward with the help of a stick, and in ten days more he left the hospital so far improved as to get up on a car unassisted. I may add, the right pupil remained contracted, which may have been due to the iritis he suffered from some years before.

In this case there was no evidence of the epileptiform attacks having been caused by meningeal irritation from diseased bone; they seem to have been induced by systemic causes—such as the syphilitic poison acting upon the nutrition in general, but on the brain in particular.

CASE III.—*Syphilis Followed by Eruption, Headache, Hemiplegia, Epileptiform Convulsions, without Loss of Consciousness.*—Esther D., aged twenty-four, was admitted into Mercer's Hospital

in the Spring of '62. She looked anxious and careworn, though of a full sanguineous habit of body. She stated that up to the period of her marriage, six years previous, she never had an hour's sickness, but from that time up till her admission she has been continually a sufferer. Shortly after marriage she contracted syphilis from her husband; then an eruption came all over body, attended with severe pains in all her joints, and excruciating headache. These symptoms continued, with short intermissions, till after the birth of her first and only child, which was born about a year after her marriage. She then sought advice, and was freely salivated. The eruption, pains, and headache were relieved for a short interval, till after fresh exposure to cold and hardship they returned, when she was again treated with mercury. About this time she got drooping of the left eyelid and strabismus of left eye, and she gradually lost power of the entire left half of the body. On admission into Mercer's Hospital she was in a most helpless condition, having been assisted to the ward by two persons. She literally had no power of the left half of the body, but there was no loss of sensation over the left side. She complained of great oppression, lassitude, and sense of weight in the back and loins, of continual headache and giddiness, of dimness of sight in both eyes; the bowels were constipated, and the menstrual function had totally disappeared for the past three years.

She got seven grains of iodide of potassium three times a day, with five grains of compound aloetic pill at bed-time. She was dry-cupped over the lumbar region, and took a hip-bath every night; her diet was liberal, but not stimulating. At the end of a fortnight she was able to walk into the garden of the hospital unassisted. At the end of a month all the distressing symptoms which she had suffered from on admission were abated, with the exception of an occasional headache, and she was meditating her departure, when, suddenly, at eleven o'clock at night of the 25th March, she was seized with twitching and convulsive movements in her extremities. She complained of intense headache, and vomited. These symptoms continued, with brief intermissions, till the time of the morning visit, when she was found to be weak and prostrate in the extreme

Ice and refrigerating lotions were applied to her head, whilst restoratives were used to the extremities. She got wine in small quantities, often repeated, and beef-tea freely. In the course of twenty-four hours all the convulsive movements disappeared; the

iodide of potassium was resumed, she gradually improved, and on the 26th of April she left the hospital, still feeling weak, but the power of the left side of the body being completely restored.

CASE IV.—Repeated Syphilitic Attacks, Followed by Secondary Symptoms—Rheumatism, Occasional Diplopia, Opisthotonos, Loss of Muscular Control, Progressive Motor Ataxy.^a

G. M., aged 48, was admitted into Mercer's Hospital on the 1st June, 1865. The following is his family history:—His father died of apoplexy, at seventy-two, and he had two sisters and one brother, who died of phthisis. In the year 1839 he contracted syphilis, for which he got mercury. About a year after, he again got primary sores, followed by a bubo, and again took mercury, and afterwards iodide of potassium. Despite of this treatment he got secondary symptoms (in the form of eruption) which disappeared under treatment. He remained in good health for about a year, when the eruption again appeared, and he then consulted the late Sir Philip Crampton, who treated him with mercurial inunction; he also took mercury, in addition, for about a month continually, but was not salivated. At this time his habits were bad, and he led a most irregular life for some months.

In 1846 he got pneumonia, from which he states that he recovered perfectly, and he continued in good health up till 1852, when he suffered from rheumatic pains over the whole body. About this time he noticed his eyesight to be first affected; when looking at an object it quivered and dazzled before him; he complained of a sensation as if something was creeping over his body, and he occasionally heard a noise as if sticks were rattling over his head and shoulders. By degrees he observed his legs begin to fail him, and he could not walk without a stick; later still, on attempting to stand up, or hold himself erect, he would have fallen backwards. He had inordinate desire for sexual intercourse, which he was able to accomplish. In this condition he continued for some years, gradually getting worse, with large abscesses forming over the gluteal region, till his admission into hospital.

On examination the following symptoms were elicited:—He complained of inability to walk or hold himself erect, owing to the tendency to fall backwards—in fact, he could only stand semi-erect, and even then only by holding steadily by some fixed point, else he

^a This case was reported by Mr. J. Myles, Clinical Clerk.

would have been drawn backwards. He complained of a sensation as if nails were driven into his hands and arms, feet and ankles. At times he seemed to have double strabismus; at other times the eyes were natural. Often he could see an object looking straight before him distinctly with both eyes; at other times unless he looked askant he could see nothing; but under any circumstances he saw best with the left eye. At night, if he looked at the gas with one eye open, the light appeared bright red, and if he looked with the eye previously closed the gas seemed nearly colourless. He was slightly deaf on the right side, and occasionally he faltered in his talk, but he had no drowsy or slow mode of speech. His bowels were costive: he had desire for frequent micturition, still the urine was acid and free from albumen; there was decided anesthesia over the soles of both feet. He was ordered to take fifteen grains of bromide of potassium three times a day, and he was rubbed over the course of the spine with a liniment composed of camphor-liniment and chloroform.

On June the 20th the following was his condition:—He was able to stand, but still had the tendency to opisthotonos. Before attempting to walk (which he did with the aid of two sticks) he rested on a chair, then started slowly and apparently with trepidation. However, his pace soon seemed to increase as if in spite of himself, and his legs were thrust forward in various directions, the feet coming down with a flap (usually with the heels first), and he extended his arms in a circuitous manner; at times he would lose his balance if not watched. He seemed to have power to move his extremities, but not to control the movements, and in this respect differed from the dragging and manifest loss of muscular action of the “paraplegic.” He complained less of the sensation of nails pricking him, and was generally improved. One-sixteenth of a grain of strychnine was now given, with the fifteen grains of the bromide of potassium three times a day, and electricity was applied along the spine. After a few days the strychnine was omitted, and the only variation in the treatment for the space of three months was the occasional substitution of the iodide for the bromide of potassium.

On the 1st of December last he could stand as erect as he ever did, and could walk five or six miles during the course of the day; still if you directed him to shut his eyes and place his heels together he would reel and fall backwards; he objected to stand for any length of time; his eyesight was more equable, but still failed him if he

overtaxed it in the least degree; there was no inequality of the pupils. In his walk he still showed want of control or co-ordination; his knees occasionally took various directions, and his feet inclined towards the ground with a flap, the heels first; and he remarked that the heel of the left boot was much sooner worn than that of the right.

I have been thus minute in the details of this case, inasmuch as this is a form of paralysis which at present is engaging no little attention. M. Duchenne, who has given this malady his special study, divides it into three stages:—The first, in which the patient suffers from paralysis, often temporary only, of one or other of the motor nerves of the eye; from some degree of amaurosis, usually accompanied by unequal pupils, and from erratic, boring pains. In the second stage, in addition to the above symptoms, the characteristic unsteadiness of gait begins to show itself, together with diminished sensibility to touch, and pain in the skin of the lower part of the legs, in the skin of the soles of the feet especially. The interval between the first and second stages varies from a few months to several years.

In the third stage the malady becomes more profound and general, the disorders in the involuntary, co-ordinate movements increase and extend, the anesthesia increases in the parts first affected, and extends to other parts, not of the skin only, but even to the muscles and joints.

In the case I have detailed the rheumatic pains were first complained of, then paralysis of the sixth nerve seemed to follow; but as there is no inequality of the pupils for so far, I infer the third pair are intact; then followed the incontinence of urine and satyriasis, the occurrence of the general paralysis being a question of some years later.

Bourdon (*Archiv. Gener.*, Nov., 1861) inclines to the belief that congestion of the spinal ganglia is the starting-point of this affection, the morbid changes being subsequently propagated to the posterior roots and posterior columns; and Charcot, Vulpian, and Dumesnil all found atrophy of the posterior columns and roots of the spinal cord.—Schmidt, *Jahrb.*, Vol. CXIV. and CXVI.

The diseases with which this form of paralysis is likely to be confounded are “diseases of the cerebellum,” general paralysis of the insane, paralysis from lead poisoning, and chorea.

Now the question may arise, why should this case come under the denomination of syphilitic paralysis? Well, in the first place, there is nothing in the family history to point to its being

hereditary further than the vague statement that the patient's father died of apoplexy at seventy-two. On the other hand, we must give due weight to the severe "sieges" of syphilis which this patient admits he suffered, and the amount of mercury he took from time to time, his habits being at the same time most irregular. Then we have a *post hoc* argument in the improvement which was brought about by a purely anti-syphilitic line of treatment; and what still further would tend to confirm the opinion as to the syphilitic character of the affection is the fact that Duchenne enumerates *syphilis* as one of the exciting causes of this malady, the others being *cold* and *masturbation*.

In the first, second, and third cases I think the syphilitic attacks were the most rational exciting causes of the nervous disorders; at the same time I do not consider every paralytic condition should be attributed to syphilis, because the patient may have suffered from syphilis in an aggravated form; still if the individual had never suffered from nervous affections of any kind, and if the paralytic symptoms occurred without any other appreciable exciting cause—and further, if their occurrence fell within the period in which the syphilitic dyscrasia generates such affections, the *fons et origo mali* may safely be termed syphilitic.

D. A. Zambaco^a concludes that every functional disturbance of the nervous system may be caused by syphilis, such as chorea, hysteria, epilepsy, insanity, paralysis of sensation and motion, neuralgia, and affections of the organs of sense.

Dr. Thomas Reade, in a very instructive paper on "Syphilitic Meningitis" (*Dub. Quart. Jour.*, Vol. XIII.), concludes that a single symptom of cerebral disturbance, such as some form of mental disorder, may alone indicate the organic mischief in progress: a cachetic countenance may excite suspicion of its nature; but a close investigation of the previous history of the patient's diseases and symptoms are required to affix its true character.

Zambaco holds that we must look still farther back, inasmuch as we meet with such affections in patients who have never contracted syphilis in a primary form; and hence the taint may be hereditary. Under any circumstances it is quite clear that, apart from any existing symptoms of the diathesis, and from the most attentive observation of the character of the nervous affections present, a most careful *résumé* of the family history is indispensable in arriving

^a On Syphilitic Nervous Affections. Paris: J. B. Baillière. 1862.

at a correct conclusion as to the origin of many of these obscure cases.

With respect to the treatment employed in the above cases, it may be said to have been confined to the use of two agents, viz., bromide and iodide of potassium. Many practitioners in such cases recommend the free use of mercury.

Dr. Duncan, in his paper on Syphilitic Insanity and Epilepsy (*Dub. Quart. Jour.*, Feb., 1863), recommends us to employ mercury without hesitation in the presence of any obvious signs of venereal action, or even whenever the previous history of the patient suggests a probable ground for supposing that such a condition may exist in a latent form; and Dr. Reade, whose paper I have already referred to, says the employment of mercury in this late stage of syphilis is unattended with those formidable consequences of which we are so strongly forewarned as forbidding its use. For my own part, if such an amount of dyscrasia or evidence of broken-down constitution, so to speak, did not exist in the cases I have detailed, I should have been inclined to give mercury a trial, but then it would have been in the form of the biniodide or corrosive sublimate in bark; but where the lineaments of rupia are present, or other such signs of constitutional break-up, I think it is better to employ the iodide or bromide of potassium, or the iodide of sodium, gold, or arsenic. Without doubt, in cases of syphilitic epilepsy, I should trust to the iodide or bromide of potassium, and such is the experience of Wilkes,^a Murchison, and others, more especially with respect to the iodide.

As regards the more special treatment of the progressive motor ataxy, Vidal (*Gaz. des Hôpit.*, 1862), Duguet (*L'Union*, 1862), Herschell (*Bull. de Therap.*, 1862), and Wunderlich (*Arch. de Heilk.*, 1863), all speak of the efficacy of the nitrate of silver in progressive spinal paralysis; but Brown-Séquard, whilst admitting its value in affections of the spinal cord, where no inflammation exists, goes on to say, that in progressive muscular ataxy the treatment must vary with the peculiar symptoms of the disease with which it may be associated. Hence in the case above mentioned, believing it to have its origin in a syphilitic taint, I adopted anti-syphilitic remedies, and for so far with the most satisfactory results.

^a Medical Times and Gazette, Jan., 1862.

ART. XV.—*On Atmospheric Conditions Influencing the Prevalence of Typhus Fever.* By THOMAS WRIGLEY GRIMSHAW, A.B.; M.B.; C.M., L.R.C.S.I.; Physician to Cork-street Fever Hospital; Lecturer on Materia Medica in Steevens' Hospital.^a

THE frequency with which typhus fever has prevailed of late, and unfortunately still prevails in Dublin, together with the well-marked character and violence of the disease, as now seen in our hospitals, affords rare opportunity for the study of this formidable disease. I do not propose to discuss the cause of typhus, whether contagion is necessary to its commencement, or whether it may arise, *de novo*, in a locality into which the seeds of contagion have not been imported, but to consider some of those causes which are favourable to its spread (and, of course, *vice versa*), and which are therefore likely to assist its origin, if it do originate, *de novo*, in a locality.

It must have been observed by all those attached to fever hospitals, and by those familiar with typhus, that at all times, and especially during an epidemic, sudden rises take place in the number of patients attacked. This is especially remarkable in hospitals, where at one time we find ourselves quickly reducing the number of occupied beds, while in the succeeding week or ten days we shall find ourselves compelled, as rapidly, if not more rapidly, to increase our hospital accommodation. On becoming attached to the Cork-street Fever Hospital, these sudden rises and falls in the rate of admissions at once attracted my attention, as worthy of inquiry as to their cause. On discussing the question with my friend and senior colleague, Dr. Kennedy, we came to the conclusion, that a sudden increase of moisture in the atmosphere, more especially when in conjunction with an increase in temperature, was always followed by sudden pressure on the hospital accommodation.

Dr. O'Brien (in his report of the House of Recovery and Fever Hospital, Cork-street, for the year ending January 4th, 1827, published in the *Transactions* of this society for 1828), when speaking of the atmospheric influences which act as exciting causes of fever, seems to hold similar opinions to those which I have just advanced—namely, that heat and exhalations from the ground

^a Read before the Medical Association of the King and Queen's College of Physicians in Ireland, on January 17, 1866.

(which are nearly all accompanied by moisture) are the chief exciting causes of fever. When treating of the causes of fever in general, having enumerated the conditions consequent on poverty, he goes on to state "that these causes alone are insufficient for the creation of such calamities, without the aid of those we call exciting causes, the most powerful of which are—extremes of heat and cold, human contagion, and malaria, or morbid exhalations from the earth, diffused through atmosphere." Further on in the report from which I am quoting, the author gives an example of how a certain condition of temperature and humidity combining may produce fever of the ephemeral class. This case, although not one of typhus, shows that this accurate observer had noticed similar exciting causes of fever to those which have been noticed by Dr. Kennedy and myself. If the case reported had been one of typhus, perhaps Dr. O'Brien would have found that, not moisture and cold, but moisture and heat were the exciting causes of the disease. It is also to be remarked that in the table of admissions into Cork-street Hospital, given in Dr. O'Brien's report, the greatest increase in admissions did not occur until the wet, or rather, variable weather commenced—namely, after the 15th of July, 1826, on which day rain fell for the first time for four months.

This opinion (of my colleague and myself) however, was founded on frequent but not very accurately-tested observation. I at once determined accurately to test the truth of our opinion by a series of minute observations, and the chief object of this paper is to bring the result of these observations under notice.

I am aware that attempts have been made to compare increase and decrease of fever with the conditions of the atmosphere, but these observations have all, as far as I can ascertain, been confined to the taking of averages for considerable periods; for instance, we read of typhus epidemics being at their height in Winter, and therefore it is concluded that the prevalence of that form of fever is favoured by cold. Again, we read of great increase of fever in the Summer and Autumn months, and some therefore infer that warm weather assists in the spread of typhus. Other writers, for instance Dr. Murchison, in his valuable and elaborate work *On the Continued Fevers of Great Britain*, very fairly concludes from those contradictory statements, that "ordinary variations in temperature have little influence over the prevalence of typhus," and says "the hygrometric states of the atmosphere have no effect on the prevalence of typhus." He seems, however, to think that cold has, indirectly, by keeping the

poor more at home, and consequently causing overcrowding in their houses, which are usually destitute of ventilation—a powerful influence in increasing the prevalence of typhus. I do not altogether agree with Dr. Murchison in laying so much stress on overcrowding, as any other effect of poverty, hunger, or dirt, might as well be taken as *the* cause, although, perhaps, overcrowding is one of the more potent causes of typhus.

It is not sufficient to take averages of months and seasons in order fairly to conduct enquiries of this sort, and draw accurate conclusions, from results thus obtained. For accurate conclusions minute observations are absolutely necessary.

In order to follow up this enquiry, I took the daily number of admissions into Cork-street Hospital during 1864, from localities situated in the south side of the city, distinguishing, during the last seven months of that year, the purely typhus cases from all others. Having tabulated these numbers, I constructed from these tables diagrams representing the prevalence of zymotic disease during the whole of the year 1864, and of typhus during the latter seven months of the same year.^a

I took the precaution of ascertaining that the rises of fever admissions in other hospitals of the city corresponded with those in Cork-street, so that the curves in the diagrams may fairly be taken to represent the variations in the prevalence of these diseases in the whole city, during the year 1864. The reasons for selecting 1864 in preference to the past year (1865) for demonstrating these observations are, that at the commencement of that year it could scarcely be said there was an epidemic in this city, while, at the end of the year the epidemic had risen almost to its greatest height. The reasons for selecting the seven months, from the end of May to the end of December, 1864, are of similar nature, with this addition, that previous to May the records were deficient, owing to changes in the hospital staff and management. But to ascertain the causes affecting the variations in the fever line of the diagrams, it was necessary first to ascertain how far back I should look for those causes, in other words, to ascertain how long ill the patients had been before admission. To do this accurately was almost impossible; but taking the patient's own statements, and making all due allowance, I found the average to be five days; and as

^a The diagrams were exhibited to the meeting before which this paper was read. The lower continuous line of the plate is a representation of the curve of the typhus diagram mentioned above.

patients usually underrate the duration of their disease, determined that I was to look back from five to six days for the causes for which I was searching. For comparison with the diagram of the rise and fall of typhus, I constructed diagrams illustrating the rise and fall of the moisture, of the minimum, maximum, and mean temperature^a of the atmosphere, for the seven months ending Dec. 31st, 1864.

I found that it was unnecessary to consider the direction of the wind, the height of the barometer, or the rainfall, as these were causes producing the variations in the former set of conditions mentioned.^b

The diagrams of atmospheric conditions thus constructed I placed in juxtaposition with that representing the prevalence of fever, placing them so that each day of the fever diagram would correspond with the meteorological conditions existing five days previous—in other words, placing the effect under the supposed causes. These I have so arranged, that they can be conveniently compared with each other.^c—See plate.

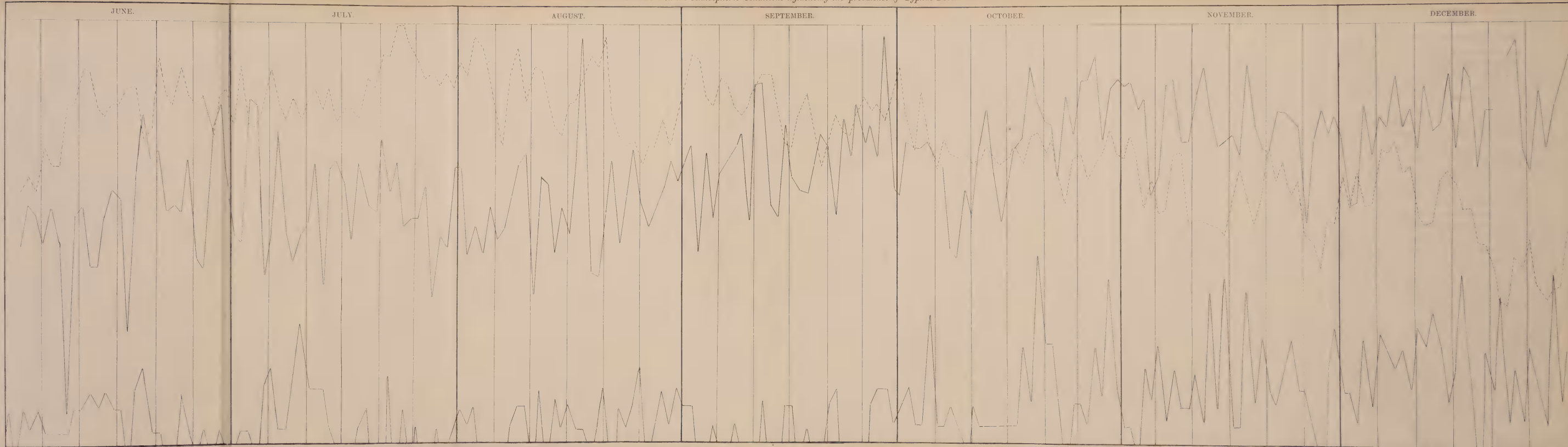
The following slight errors occur in the diagrams:—

Sundays and Mondays cause an inequality in the rate of admissions, being usually less on Sunday than on any other day during the week, and consequently more on Monday; also the month of September is incorrect, as only about half the admissions from the 3rd of that month to the 3rd of October are represented; this, however, does not influence the *relations* of the rise and fall of the line to the conditions of the atmosphere. I thought it better to leave the error as it is, mentioning it, than attempt a theoretical correction. Before carefully comparing the diagrams when finished, I felt so convinced that the moisture was the great influencing

^a The dotted line in the plate represents the variations in mean temperature, the upper continuous line the variations in moisture. It was considered better not to place all the curves mentioned above, in the plate, as only likely to cause confusion. The vertical lines in the plate are drawn at intervals of five days.

^b At the conclusion of the reading of this paper before the Association of the College of Physicians, Dr. Lyons suggested to the author the propriety of adding a line to his diagrams, representing the variations of the amount of ozone in the atmosphere. To do this with regard to the present series of observations was impossible, as the author had no table of ozonometric observations to refer to corresponding with the period over which his observations extended. The author hopes, at some future period, to be able fully to investigate this interesting question.

^c A tracing combining the curves of these diagrams on one sheet, placed on a perpendicular board, with a plumb line attached, so that all could be compared at once, was exhibited at the time this paper was read.



The dotted line represents the rise and fall of mean Temperature: the upper continuous line the rise and fall of wetness in the atmosphere: the lower continuous line the rise and fall of barometric pressure from May 30th to December 31st 1861.

cause, that I tested my opinion by giving the fever diagram to a friend, and taking the moisture diagram myself. In taking sixteen instances at random, I stated what variations should be found by my friend in the fever diagram, and in all those instances I proved correct. What conclusions may we draw from the inspection of these diagrams?

1st. That an increase in the moisture of the atmosphere favours an increase in typhus, and *vice versa*.

2nd. That an increase of temperature favours an increase of typhus, and *vice versa*.

3rd. That the two previous conditions combined are *most* favourable to an increase in typhus, and *vice versa*.

5th. That when cold and moisture combine, the former tends to diminish the influence of the latter.

It will thus be seen that an increase of moisture, other things being equal, is the atmospheric change most favourable to an increase of typhus; but that when to this is added an increase of temperature, we have *the* most favourable atmospheric conditions for the spread of fever. It would also appear that dryness and cold (the latter particularly) have an influence contrary to the spread of fever. I do not mean to state that there are no exceptions to the rules just laid down, but the exceptions, which are few, and by no means well marked, are easily accounted for by other disturbing causes.

Since the greater portion of this paper was written, I have had *three* opportunities of verifying, in a remarkable degree, that an increase of moisture in the atmosphere, with a rise in temperature, is very favourable to an increase in fever; namely—

On December the 7th, 1865, an increase of moisture and temperature, which was observed and noted at the time, was followed by an increased number of admissions into Cork-street Hospital. On the 12th and 13th of that month the number of patients admitted was eighteen; the average for that year being six per day.

On Christmas-day, 1865 (the peculiar atmospheric conditions of which must be remembered by all), I took another note to the effect that there should, in consequence of the unusual dampness and heat of that day, be a pressure on the hospital accommodation on January 1st, 1866; this proved to be the case.

The last instance, however, is the most remarkable. On January 5th the hydrometric condition was unusually high (reported 0·9 at the Ordnance Survey Department); the temperature also was rather

high during the day, but not very remarkably so. I foretold a great rise in the hospital admissions on the 10th and 11th, which took place—21 patients being admitted on those two days, with a few on the morning of 12th, altogether more than I have known to be admitted in the same short period since my observations commenced, the rate of admission on the 11th, being the highest but one of which I have taken particular note, although I think there may be a few as high.

I have foretold a rise in the rate of hospital admissions on the 18th of this month (January), in consequence of observations taken on the 13th of January (the day following the late frost). I should like the gentlemen present, connected with fever hospitals, to observe if this forecast proves correct.^a

I think these observations of some practical value, as all cause favouring the spread, must necessarily favour the origin of disease.

ART. XVI.—*Remarks on Diphtheria, chiefly with reference to its Contagious, Epidemic, and Fatal Character; and to its Supposed Connexion with the Cattle Plague, and with a Certain State of the Weather; founded on the Details of Several Recent Cases.* By T. W. BELCHER, M.A. and M.D., Dublin; Fellow and Censor of the Royal College of Physicians in Ireland; Physician to the Dublin Dispensary for Skin Diseases; and sometime Physician to the Cork Fever Hospital.^b

THE following observations are laid before the profession, not because of their originality, for no claim is here made to anything of the kind, but solely because of the practical importance of any well ascertained facts respecting so fatal a disease as diphtheria.

The fact, often noted by observers, that the more fatal varieties of this disease are not commonly met with in hospitals renders it of greater moment that such cases as are met with in private practice should be duly recorded.

CASE I.—A lad, aged about fourteen, a boarder in a large school in this city, was attacked, shortly before Christmas, 1865, with an

^a This forecast *did* prove correct, a large increase in hospital admissions taking place on the 18th and 19th of January, 1866.

^b Read before the Medical Society of the College of Physicians, Dublin, 21st February, 1866.

acute affection of the throat, accompanied by morbid exudation on the mucous membrane of the pharynx. He was attended in the first instance by Dr. Duke, of Harcourt-street, who subsequently called in Dr. Wharton in consultation. I understood Dr. Duke to say that the case presented the appearance of gangrenous ulceration, though, from the subsequent cases about to be detailed, he is now of opinion that it was one of diphtheria. It is quite possible that both opinions are correct; indeed, M. Trousseau asserts that in certain rare cases gangrene does affect the parts attacked with, what he terms, "Diphtherite." He observed this occurrence twice, and states that the sphacelus was confined to a small portion of the velum palati in one case, and of the skin of the arm in the other.^a In the present case there was considerable swelling at the left side of the neck. This lad became convalescent, and was removed to Sandymount for change of air. A few days after his removal he complained of great prostration and nervous depression; head symptoms, like those of effusion of the brain in children, supervened; and despite of every care he rapidly—almost suddenly—sank, and died in what seemed to ordinary observers an unaccountable manner. There was not any *post mortem* examination of the body.

CASE II.—Shortly after the occurrence of the illness in Case I.—but some time before the death in that case—a young man, aged twenty-four, a tutor in the same school, where he spent some hours daily, though he did not reside in it, became affected in like manner. His case was pronounced to be diphtheria by one of our most eminent surgeons, who attended him. He apparently recovered, but suffered from prostration of strength. Food he could, and did take plenteously; and he talked of every passing event with his usual interest. Suddenly he one day became exhausted, and rapidly sank, to the consternation of his family, who looked on him as convalescent.

CASE III.—A lady, aged about thirty-six, the wife of the governor or principal of the school already referred to, and the mother of several children, had assiduously nursed Case I. through his illness in the school, where she also resided. After his death she continued for some days apparently well, until the 28th of December, 1865, when she complained of rigors; general febrile

^a Trousseau on Diphtherite. Sydenham Society's Memoirs on Diphtheria, p. 237.

symptoms supervened; and these were again followed by every symptom of cynanche tonsillaris. The local affection was confined to the left side of the fauces.

She was attended by Dr. Duke of Harcourt-street, and on the 30th Dec., Mr. Adams was called in in consultation. These gentlemen pronounced the case to be one of unequivocal diphtheria. I saw her, with Dr. Duke, on the same evening, and I was of a like opinion. The colour and consistence of the false membrane were of the kind usually described in books; the local affection, however, as already remarked, was confined to the left side of the fauces; the left side of the neck was swollen; and I cannot better describe the tumour than by adopting the description of a case of Dr. (now Sir Dominic) Corrigan, from a clinical lecture of his, published in the *Dublin Hospital Gazette* for 15th February, 1859:—"His neck was swollen out into oval form from anterior third of clavicle to middle of neck, the oval swelling then bending inwards . . . as it ascended, so as to leave unaffected the hollow under angle of jaw." In Dr. Corrigan's case the tumour was on both sides of the neck, but in other respects the present case corresponded exactly with his. The false membrane did not extend down the pharynx; her power of swallowing was not materially interfered with; her voice not at all affected; she complained of distress rather than pain; the pulse weak and frequent; the skin acting freely. She had been taking Dover's powders combined with nitre, wine, beef-tea, and ten drops of the tincture of the perchloride of iron every third hour, in water. At Mr. Adams' visit in the morning he had removed part of the false membrane, but in the evening the same place was covered over afresh. On the 31st of December, the tincture of iron was applied internally; the exudation had evidently extended to the nares, but not to the pharynx, so far as we could judge. After the first application of the tincture of iron she discharged her stomach, and became much prostrated. She now got plenty of brandy. The urine was copious and apparently free from lithates, but I had not an opportunity of analyzing it. The pulse became very weak and frequent; and the peculiar offensive odour from the breath was very perceptible, although I had a cold at the time. The tumour of the neck continued to increase, and I noticed over the upper lip, at the left side, a well developed patch of herpes. In the evening the disease had so far extended as to prevent her breathing through the nares. A solution had been prescribed in the afternoon for application to the false membrane; this consisted

of three drachms of hyposulphite of soda, two drachms of glycerine, and six drachms of water; to a certain extent this solution—which was recommended in the *Medical Times and Gazette* of the previous day—had proved useful. It relieved her very much, and enabled her to throw up pieces of the false membrane. It was now directed to be continued.

At 9.30, a.m. on the 1st of January, 1866, on my arrival at the house I found that she had just died. In the course of the night her brother and her nephew, two judicious medical practitioners, had come from the country to see her. They saw her about 5 or 6, a.m., and from one of them I learned that she then had thrown her head backwards—as a child does in croup—and seemed to breathe with difficulty. The difficulty of breathing did not, however, seem to be great; and so they went to bed. They again arrived at the house shortly before her death, and found her almost asphyxiated. Tracheotomy was not performed. There was not any examination of the body; but we concluded that death had resulted from extension of the false membrane into the larynx. I am informed that her pulse continued to beat strongly and regularly for three minutes after she had ceased to breathe. She had had scarlatina many years before; but, so far as I know, none of the other persons whose cases are here noted had suffered from that disease.

CASE IV.—When the lady described in Case III. got ill, her daughter, aged about seven, was sent out of the house. Almost at once she complained of illness, was brought home again, and exhibited the unmistakable signs of diphtheria. In this case, which I attended with Dr. Duke for several days, the false membrane was chiefly on the right side of the fauces; there was a tumour of the neck of the same shape as in the mother, but on the right side, while there was a well defined patch of herpes on the left side, over the upper lip. Typhoid symptoms prevailed here; and for days the child was in the balance. More than one relapse took place; and when she became almost convalescent the tumour in the neck suppurated and caused a troublesome abscess, which is now healed; although it, for some time, left her very anemic and weak. She had the peculiar odour of the breath noted in Case III. I tested her urine, and by the ordinary tests of heat and nitric acid obtained a very large quantity of albumen, nearly half in volume as compared with the volume of the urine tested.

In this case the main principle of treatment was support by food, wine, and bark, with tincture of iron applied locally, and administered internally. She had epistaxis several times, and delirium was a prominent symptom in her case. Some weeks after her apparent convalescence she exhibited signs of great physical prostration; but now (20th February) these have been overcome to a considerable extent. She has still (21st February, 1866) to contend with the effects of disordered innervation, as evidenced by injury to the powers of speech and deglutition, and defective vision.

CASE V.—When the child just referred to was removed from her father's house, she went to that of an aunt, and there slept for a night or two with her first cousin, a girl about her own age. In a few days the latter complained of sore throat and general *malaise*. I saw her; and perceiving that there was a general family panic—for the aunt had meanwhile died—I advised her removal from the house. This was done; she was well cared for; and in a few days she returned home quite well. No one else in that family got ill, so far as I know.

CASE VI.—A female servant of the lady whose death I have recorded, aged about twenty-one, got ill during the illness of her mistress, and so continued for several days after the death of the latter. I attended her also at the school with Dr. Duke. Her case was one of diphtheria, with the peculiarities of those already related. The tumour was at the left side of the neck, and the patch of herpes, which was also at the left side, was large and well defined. She used a gargle of tincture of the perchloride of iron, one part to three parts of water. This proved a most useful application, and detached the false membrane in large quantities. She recovered, but still (20th February) retains some of the most striking sequelæ of the disease; she talks "through her nose," the irregular action of the pharyngeal muscles causing food to return through the nares. In fact her powers of speech and deglutition are seriously impaired; and with this she also labours under considerable defect in the power of vision.

CASE VII.—Immediately on the occurrence of all this illness in the one house, the head of the family had his other children removed to Sandymount, where, in about a fortnight, his son, a

lad aged about twelve, was attacked with diphtheria. In his case the tumour of the neck, and the false membrane for the most part, were on the right side; and unlike the others, there was here no appearance of herpes. Dr. Duke continued to attend him to his convalescence. He tells me he is now quite recovered.

CASE VIII.—Another child of this gentleman, a girl aged about five, contracted sore throat in the same house at Sandymount. She recovered without any symptom of diphtheria.

CASE IX.—Another daughter of the same gentleman, aged eight years, got diphtheria about a fortnight after her mother's death, in the same house at Sandymount. She was ill at the same time as her brother whose case has been referred to (Case VII.) She had not any appearance of herpes, but had the tumour at both sides of the neck. Dr. Duke attended this case to the close, and he tells me that she recovered sufficiently to be able to take daily walks. One day at the close of last month she complained of great weakness, after a short attack of diarrhea. She sank, suddenly almost, and notwithstanding every effort, she died.

CASE X.—A clergyman who was frequently in and out of the school-house during the occurrences already mentioned, became unwell; got sore throat and general *malaise*, and it was fully a fortnight ere I could say he was well. In that case there was a slight greyish, apparently vesicular, exudation on the fauces.

CASE XI.—A gentleman who came from the neighbourhood of this city to the funeral of the lady referred to in Case III. informed me that after leaving the house where she had died he at once felt ill. I found his throat slightly congested, but after the use of a gargle he quickly recovered, without any further bad results.

CASE XII.—The nurse who attended the lady that died had to leave the house with sore throat and general *malaise*. In a few days she recovered.

Nine boarders at the school to which I have referred became more or less affected with what, from the description given to me, seems to have been diphtheritic sore throat; but most of them recovered on going into the country at Christmas. Not so,

however, with one, who, I have just heard (20th February) is suffering from partial paralysis of the motor power in the upper and lower extremities, and almost total loss of vision.

Beside the above cases, Dr. Duke became unwell with sore throat, and the same happened to me. A servant of mine, who went to the house with a message from me, was similarly affected; so was a lady who called there; and lastly, the gentleman in whose family all this occurred got sore throat, but soon recovered. I myself, and most others who entered that house at this time, felt conscious of a very unpleasant smell, which immediately suggested the idea that the sewage was defective. The house, which is a very fine one, was cleaned, papered, and painted; the kitchen was investigated, and under it was found a sewer which, in the opinion of some, accounted for the smell. In that house about eighty persons had lived; and fortunately most of this illness occurred during the Christmas vacation. This smell cannot now be perceived.

On the preceding cases I may remark:—

1. That several members of the same family, with others more or less connected with them by occupation, contracted diphtheria or diseases allied thereto.

Just as when cholera is present or is approaching we find cases of diarrhea very common, and some of them not easily separated from cholera; and just as when some members of a family have scarlatina while others have sore throat only; so, to quote the words of Dr. Jenner, “One or more members of a family having exudation on to the pharyngeal mucous membrane, and others at the same time having merely inflamed throats, would be strongly presumptive evidence that the latter had diphtheria without exudation.”^a

2. It may be further observed that these cases were (at first) limited to one house. Dr. Aitken has remarked a similar limitation;^b while the circumstances already detailed would point to the conclusion that not only was the disease infectious, but that the miasma generating it was localized.

I inquired of several physicians in this city at the time that these cases occurred, and I could not find that diphtheria was at all general; very far from it. One physician told me that he was

^a Diphtheria. By Wm. Jenner, M.D. P. 15. Lond. 1861.

^b Practice of Medicine, Vol. i. Edition, 1865.

then attending one bad case, in private; and the next thing I heard was that he himself was ill in diphtheria. Dr. (now Sir Dominic) Corrigan has remarked, in the lecture already adverted to, that an epidemic is mostly ushered in by dropping cases scattered here and there. Those who recollect the last fever and cholera epidemics in this country will bear him out in the truth of this remark; and they will also agree with me when I recall to their minds that in these epidemics some cases, to all human appearance, resulted from miasmata generated by bad sewage, while others had no possible connexion with any such cause. Just so with diphtheria; an epidemic is preceded by scattered cases. Any one may contract it; but those exposed to infection or to miasmata injurious to general health are specially susceptible of it. Since these cases occurred I have heard of others quite as scattered, but more frequent. From all this, taken into connexion with the history of epidemics, perhaps it is not rash to expect a general visitation of this disease. Whether it has any connexion with the peculiar state of the weather this Winter would be mere guessing; but I should like to ascertain how far previous epidemics of diphtheria were associated with weather such as we have had for some months past.

3. The tumours described in these cases are worthy of remark. In no case was the neck swollen, as in mumps, over the superior portion of the parotid gland; nor did the tumour involve the inferior angle of the jaw, as in scarlatina or cynanche. Dr. Corrigan noted this peculiarity, and evidently considered it to be a sign of value.

4. The occurrence of herpes in diphtheria I have not seen noted by any writer. Dr. Jenner indeed says that French writers describe an herpetic eruption on the mucous membrane of the pharynx, which may be mistaken for diphtheria. "It is commonly associated with herpes of the lip, and, as a rule, is much more painful than diphtheria, the pain being limited to a single spot in the pharynx."^a

The cases to which I refer—those of unmistakable diphtheria in which there was herpes on the lip—had no appearance whatever of herpetic eruption on the mucous membrane of the pharynx. The clergyman (Case X.) who had a vesicular exudation on the fauces had *not* herpes on the lip; and, since the above cases occurred,

^a Jenner, *op. cit.*, p. 61.

I met with another, in which a clergyman had a vesicular eruption on the pharynx preceded by partial aphonia—he had not herpes on the lip either; though in other respects his case resembled those noted by the French writers and by Dr. Jenner.

Further, a gentleman whose son was a day scholar at the school previously referred to, informed me that the latter went home with sore throat from it. This soon disappeared during the Christmas vacation, but a large patch, of what, from his description, seems to have been herpes, appeared on the forehead over the left eye.

5. The occurrence of sudden death, or what nearly amounted to it, in so many cases is worthy of note. In the cases of this kind recorded by Dr. Jenner the cause is said to be asthenia, while Dr. Aitken states that death sometimes occurs from syncope. Nearly a century ago Dr. Rutty^a remarked of an epidemic which prevailed in Ireland in 1743, when the weather was very much like ours during the last year, that it in some points resembled Fothergill's sore throat. Of the cases then occurring he thus writes:—"Their death is generally sudden, and when least expected, the pulse being good a minute before."

In the cases above detailed apparent convalescence was plainly no guarantee of safety.

6. Like other cases recorded, there has been here partial paralysis of the muscles of deglutition as well as injury to the powers of speech, vision, and generally disordered innervation.

7. The fact of the pulse beating for three minutes after breathing had ceased is not common, but that it may occur Sir Benjamin Brodie has proved in his *Lectures on Pathology* (Lecture 66).

With reference to these cases two practical questions suggest themselves: (a) should the false membrane be torn off; (b) should tracheotomy be resorted to?

(a) Dr. Jenner, and most other authorities, decide against tearing off the false membrane; "to do that (says Jenner) is to commit a decided blunder." They consider that mechanical irritation of this kind aggravates the disease; while the immediate reproduction of the false membrane on the abraded surface shows the operation to be of little practical value.^b It is quite a different matter to mop the diseased surface with a strong preparation such as tincture

^a Chronological History of the Weather, and Seasons, and Diseases Prevailing in Dublin. Lond., 1770.

^b In a case of putrid sore throat Fothergill notes the reproduction of the slough after mechanical separation (Works, p. 239).

of the perchloride of iron, with a view to dislodge the membrane, and so enable the medicine to have at once a local and a constitutional effect. The same end, however, will, so far as I have seen, be answered by using a strong gargle of the same tincture (see Case VI.).

(b) Whether tracheotomy, or, in the case of adults, laryngotomy should be resorted to, to prevent asphyxia, is a grave question.

If the patient die immediately after it, the physician who advised and the surgeon who performed it will scarcely be held blameless by the friends of the patient, who, even when life is fast ebbing, cling to hope. On the other hand, to save even *one* life out of many by such a means is so great a success that most persons will think it worth the venture, especially as successful cases now and then occur. In any case where there is blood-poisoning, and where the friends object to the risk, I do not think it would be wise to urge it; but if the friends of the patient accept the risk promptly, in a case where the operation would seem to indicate success, the opportunity should not be let slip. In Case III. of those now detailed I am not able to say what should have been done, as I did not see the patient during the approach of asphyxia; but from the character of the medical gentlemen who did see it, and their practice in other like cases, I conclude that it was not a case likely to be benefited by operation. The blood-poisoning was evident, and the case was a bad one from the outset.

I believe Bretonneau had a *primâ facie* case when he contended for the identity of what we now call diphtheria with croup; especially since Jenner and others have recorded cases in which the false membrane in diphtheria extended into the air passages. I do not, however, propose to raise this question at present; because, from the nature of the diphtheritic exudation and other symptoms, we are all now pretty well agreed on the important and essential differences between the two diseases; but inasmuch as I have observed in a very recent medical work—the newly abbreviated edition of Copland's Dictionary—that Bretonneau is considered to have erred in separating diphtheritis from gangrenous sore throat; while in the same article Fothergill and Huxham are stated to have described diphtheria, in 1748, thereby making it and putrid sore throat identical, I think it may not be out of place to endeavour to show that Bretonneau was right after all; and that Fothergill certainly, and Huxham probably, never described what we know by the term diphtheria. Almost up to the present hour

“Fothergill’s sore throat” and scarlatina, or cynanche maligna, have been looked on as one disease, and classed in books accordingly. Taking Fothergill’s own description, we find that the exudation, in the English epidemic of 1748, was white or ash-coloured, and then black—it sloughed away leaving gangrenous ulcers. “The redness of the skin in the face, neck, breast, and hands is another obvious and distinguishing characteristic, which in children, and young people especially, seldom fails to accompany this disorder.”^a It is scarcely necessary to say that this description will not fit diphtheria, as we see it; while it undoubtedly will fit scarlatina maligna, the cutaneous eruption of which is such as is not met with in diphtheria; although in some cases of the latter there may be slight gangrenous ulceration, as noted in Case I. Huxham noted the “gangrenous sloughs” in much the same terms as Fothergill; and had he not, in addition to the symptoms recorded by the latter, noted the formation of an exudation analogous to that in the throat on abraded surfaces on other parts of the patient’s body, it could not be said that he made even an approach to describing diphtheria. It will, perhaps, be said that he described the cutaneous diphtherite of Trousseau;^b but as he dwelt specially on the scarlatinal eruption we may safely endorse the dictum of Bretonneau, who says:—“The gangrenous sore throats observed by Huxham are also related for the most part to scarlatinal angina.”^c The exceptional cases recorded by Huxham and Fothergill may lead some to believe that diphtheria was the disease described by them, particularly as Fothergill quotes the old writers as describing his disease, while the writers of our own day quote some of these very descriptions as applicable to diphtheria; but if this lead to any logical conclusion it must be to that which now-a-days finds many advocates:—That all this class of zymotic diseases proceeds from one poison, which operates variously under varying conditions; and that these diseases are not so many separate entities “like dogs and cats,” as Miss Nightingale expresses it; but that under various names we may have essentially the same thing; some important symptoms being at one time prominent, and in their turn giving place to others. That something like diphtheria was observed at this time must be allowed;^d and

^a Fothergill’s Works, p. 225. London. 8vo. 1785.

^b Trousseau.—Sydenham Society’s Memoirs on Diphtheria.

^c Bretonneau.—*Op. supra cit.* First Memoirs.

^d Even in our own day Dr. Jenner writes of his own experiences:—“Diphtheritic inflammation of the pharynx sometimes complicates scarlet fever.”—*Op. cit.*, p. 57.

if we consider that then, as now, the cattle plague prevailed in England, and that the weather preceding or accompanying the pestilence was unusual, and not unlike what we have lately experienced, I think the coincidences are somewhat striking. In an old pamphlet, published anonymously, in London, in 1748,^a a physician writes to the Privy Council:—*An Essay concerning Pestilential Contagion, occasioned by the Distemper now raging among the Cattle, with a Method proposed to Prevent its Progress.* This essay is a curiosity in its way, and gives so much information about the progress of the cattle plague at that time, of its great cost to the country, and of the means proposed to arrest it, that it might well be written at the present day. In the course of the essay there is a concise summary of the various pestilences known to history; at the close of this the writer proceeds:—"I will conclude this part of my enquiry with an account of the *Pestilent Angina*, a distemper that raged at London, in the Winter of 1739. The best relation I could at that time procure was as follows:—It began with a slow fever, and soreness of the throat, and generally in about twenty-four hours there appear'd a small white speck or two upon the tonsils, which, if left to themselves, encreased in number, grew livid about the edges, and, running into one, formed a large slough all over those glands, and spread gradually over the uvula, and all the arch of the throat. In a few days a mortification began, and carried off the patient before danger was thought on; for neither the fever nor pain in the throat were violent all this while." This description, in which no mention of the scarlatinal eruption occurs, is obviously nearer to diphtheria than that of Fothergill. That it had a certain relationship to Fothergill's sore throat may be inferred from the fact, that the anonymous physician, just quoted, proceeds to give the descriptions of Aretæus, and also of Severinus, a Neapolitan of fame, in the seventeenth century, as applicable to *his* delineation. Fothergill quotes these identical authorities with an analogous view; and so do most of our modern writers on diphtheria. The anonymous writer, whom I have already quoted, proceeds to affirm that:—"The affinity is certainly very great between the pestilential sore throat and the present disease of the cattle;" and proceeds to give extracts from authorities in support of his view. Were this a paper on the cattle plague these extracts might be here given, but it may suffice to

^a This pamphlet may be seen in the Library of the College of Physicians, where I became acquainted with it through my official connexion with that Library.

note the important fact, that, previous to or during an epidemic, domestic animals have suffered from analogous diseases. Dr. Greenhow has fully dwelt on this in his treatise on diphtheria, pp. 118, &c.; and from the early periods of Roman History no historical statement is better established. Fothergill (writing in 1748) states that the angina maligna showed itself most frequently in Autumn, and the beginning of Winter, *i.e.*, from September to December inclusive. He also "Remarked that the Summers of 1747 and 1748 were dry, with some days, in each, uncommonly hot, for this climate—the mercury in Fahrenheit's thermometer rising in the shade, and within doors, one day to 78, and during several to 75 and 76. The Autumns of the same years were as unusually temperate and warm; the wind continuing longer in the southerly points than has often been known at this season." I believe this description will answer pretty well to that of our last Summer and Autumn; and from all these circumstances I am led to expect that diphtheria, and diseases allied to it, will very shortly become epidemic in this country.

Dr. Jenner has noted the *constitutional* nature of this disease; its tendency to attack blood relations though separated by long distances from each other.

Since writing the former part of this paper I have had further information which not only illustrates the contagious nature of this disease, but also, to some extent, confirms the doctrine of Jenner to which I have just referred.

I had occasion to visit a large town in the west of the County of Cork; and there I ascertained that while the lady referred to in Case III. was ill in this city, her nephew, a young man of about twenty-two years of age, contracted diphtheria at a distance of about 240 miles from her. They had no communication with each other, nor at that time was there any other case of diphtheria in the town of which I write. This young man recovered; but his sister, a child, who lived in the same house, contracted the disease, with symptoms precisely similar to those noted in Case III. When in imminent danger of asphyxia, tracheotomy was performed on her by one of the provincial practitioners, to whom reference has previously been made. It relieved her very much; quantities of the diseased membrane of a buff colour were voided through the aperture, but she died from asthenia in eighteen hours after the operation.

Further still.—A young lady, a cousin of these parties, living in the same town, but having no communication with them during

their illness, contracted diphtheria about the same time, and died of it. In her case the disease was presumably traceable to the fact that a niece of the lady in Case III. had left the house in this city on the occurrence of her aunt's illness and had gone home, taking with her a sore throat. She slept with the young lady of whose death I am now writing.

In both these instances the apparent propagators of the disease recovered, while those apparently taking it from them died. There were no other cases of diphtheria in that neighbourhood, save in a small island some miles off the coast of Schull, where there were five cases. Since the occurrence of the above deaths, epidemic sore throat seemed to have prevailed to some extent. Immediately on my going to the town of which mention is now made, I fell ill with ulcerated sore throat, plus an ash-coloured pharyngeal exudation, which a sound practitioner in that neighbourhood burned out in three days by applying to it a strong solution of nitrate of silver.

ART. XVII.—*On Bromidrosis or Fetid Perspiration.* By ARTHUR WYNNE FOOT, M.D., Fellow of the King and Queen's College of Physicians.

THE term Bromidrosis ($\beta\rho\tilde{\alpha}\mu\omicron\varsigma\acute{o}$, a stink, especially of beasts at rut, $\dot{\iota}\delta\rho\acute{\alpha}\varsigma$, sudor) is employed to signify that, under various conditions the secretions of the skin which in health are almost inodorous become remarkably tainted and offensive; the circumstances under which this phenomenon appears are various, and may be conveniently arranged into three groups, each worthy of a short consideration. The first group of cases are those in which the smell from the skin is the result of the introduction into the body, for the purposes of food or medicine, of some substance which may be itself inodorous; the second group of cases are those in which the smell from the skin indicates disease, existing and progressing internally, some of whose products or results are carried outwards, dissolved in the perspiration or sebaceous secretion; in the third group of cases the cause of the phenomenon is generally unknown or very obscure, which fact is one of the reasons that this form, which is not the least frequent of the three, is often long and unsuccessfully treated. The perspiration secreted in health from the surface of the body, when cleanliness is as scrupulously attended to as it ought to

be, is almost inodorous, or at least rarely appreciable by the organ of smell, as constituted in the higher mammals, so that it has long been the practice of physicians to look upon any odorous condition of this secretion as a symptom of ill health deserving their serious attention; for this reason Paulus Egineta has handed down four prescriptions for fetor of the perspiration.

Examples of the first class of cases, in which the fetor of the perspiration is dependent upon certain articles used as food or medicine, are sufficiently common; the fetor in these cases ceases when, or soon after, the cause is removed; there is no disease or illness necessarily in the case, and the worst result is that the individual becomes personally and publicly obnoxious. Aristophanes alludes to the disgusting emanations from Asiatics who consume assafetida daily. The volatile oils contained in this gum-resin, as well as those contained in the genus *allium*, including the garlic, onion, shallot, and leek, being absorbed by the veins, taint the exhalations and secretions of those who use them. The perspiration of the inhabitants of parts of the Arctic regions, as the Samoiedes and Esquimaux, smell strongly of whale oil; this animal oil, often not in its freshest state, forming their chief food. The leguminous odour of the sweat of Greenlanders is attributed to the quantity of peas they eat. During the administration of phosphorus, sulphur, and tellurium, the compounds of hydrogen with these substances are emitted from the skin in an unmistakable manner. That the exhalations from the lungs are not alone impregnated with these gases is proved by the fact that silver articles become blackened in the pockets of persons undergoing a course of sulphurous medicines. The results of the union, within the body, of hydrogen and tellurium, are so offensive that it is improbable that this substance, first used medicinally by Sir James Simpson, will ever be a favourite remedy. The odour of telluretted hydrogen cannot be distinguished from that of sulphuretted hydrogen. Dr. Aquilla Smith has recorded (*Dub. Quart. Jour. Med. Sci.*, Vol. XVI., p. 194) the effects he witnessed subsequent to the administration of the nitrate of tellurium in some experiments on its action, undertaken at the suggestion of Sir James Simpson. Tuesday, August 31st, 1852, at noon, one half-grain pill was administered to each of the following patients in Sir Patrick Dun's Hospital:—Christopher Connor, aged 32, under treatment for chronic pleurisy, and John Shalvey, under treatment for a venereal eruption. The two patients lay in beds about five feet apart, in a spacious and well ventilated ward. September 1st.—

A peculiar odour was perceived from Connor's breath, but none from Shalvey. A pill was administered to each patient at noon. September 2nd.—An alliaceous odour was very perceptible; two physicians noticed it immediately on entering the ward. A third pill was administered to each of the patients at noon. September 3rd.—The odour to day was very strong, and so offensive that the nurse and the other patients in the ward complained of it; the two patients also were very sensible of it, and remarked that they had a very disagreeable taste in their mouths. Shalvey, who was a sailor, said the taste was like garlic. One of the physicians, who saw the patients the day before, was so disgusted that, on entering the ward to-day, he retired immediately. The odour was very like that emitted by metallic arsenic when volatilized. Each patient having taken three pills, and the effect being so palpable, Dr. Smith did not consider it necessary to pursue the experiment. On questioning the patients minutely, he could not ascertain that any other particular effect had been produced by the pills. Shalvey said, they made him perspire more than usual. September 4th.—The smell through the ward was diminished, but the exhalations from the patients' lungs were very offensive. September 5th.—The peculiar odour less evident on entering the room. September 8th.—Connor's breath still retained the peculiar smell very strongly; Shalvey was quite free from it. The latter patient, during the experiment, did not emit so strong an odour as Connor. The increased perspiration may have eliminated the tellurium more rapidly than in the case of Connor, who did not perspire more than usual. September 16th.—Connor's breath still retains the odour, which is not perceptible when standing close over him.

In the second group of cases, an ill smell from the skin is a symptom of disease, obvious and serious, and they deserve a passing notice. A fœtor is observed not unfrequently from the skin in lunatics as diagnostic as that from a variolous patient, it may be perceived on entering a room where the person has been for some hours, and especially during the night. Dr. Tuke remarks that the room smells like a mouse trap. In the fourth visitation of the sweating sickness, the smell of the perspiration is described as "odoris teterrimi." During this epidemic in the course of a short time, and in many cases at the very commencement of the disease, the "stinking sweat" broke out in streams over the whole body. In the next visitation it was described as being thick and of various colours, but in all cases of a very disagreeable odour, which

when it broke out again after any interruption to its flow, was far more offensive than at the first, so that the body dripped as it were with a foul fluid, and it seemed as if the inward parts wanted to disburthen themselves at once of their putridity by an immoderate effort (Hecker, *Epidemics of the Middle Ages*, p. 260-1.) An unpleasant and peculiar smell is given off from the skin in typhus fever after the first week, which exhalation Murchison is inclined to believe to be a chief vehicle for the transmission of the typhus poison, and with which some fever nurses are so familiar as to be able to distinguish typhus from typhoid fever by the smell alone. This smell was noticed three centuries ago by Salius Diversus; it has been frequently alluded to by subsequent writers, by whom it has been compared to the "odour of rotten straw," or to "the disagreeable affecting scent from a person labouring under the confluent small-pox," to the smell given off by deer, or by rubbing the leaves of rue between the fingers, and to the smell of mice (*Murchison on Fever*, p. 134). Whenever perspiration, sensible or insensible, occurs during the active dissolution of the morphotic constituents of the body, it bears more or less the odour of the putrescence which is going on internally, thus during electrization of a limb affected with spontaneous gangrene, a fetid perspiration has been observed over the electrified part (*Gaz. Hebdom.* Jan. 28, 1859). The extreme fetor of the sweat in the sweating sickness may also be thus explained, as in that disease decomposition was most rapid, and the cadaveric odour so intense that the bodies of its victims had to be buried in graves unusually deep (Hecker, *op. cit.*, p. 262, note). Dr. Donovan writing of the effects of the famine of 1846-7, among the peasantry of Skibbereen, describes the skin as exhaling a peculiar and offensive fetor, and being covered with a brownish filthy-looking coating, almost as indelible as varnish, which he was at first inclined to regard as incrustated filth, but which further experience convinced him was a secretion poured out from the exhalants on the surface of the body, (*Dub. Med. Press.* Feb. 2nd., 1848, p. 67). Murchison also remarks that other observers have noticed that during starvation the body exhales a putrid odour, not unlike that of a corpse, and that after death putrefaction is immediate and rapid. Among other examples of odorous perspiration depending upon constitutional causes, and indicating serious disease, are the sour-smelling sweats of rheumatic fever, and the amoniacal perspiration, of the face especially, in disorganization of the kidney. The fetid secretion from the skin which attends

eczema mercuriale, mucous tubercles (although the sweat of syphilitic people is said to smell sweet), and favus, is not solely due to perspiration, as the skin is in such cases more or less broken, but to the fluids formed on the surface of superficial ulcerations.

The third group of cases in which the cause of the ill odour of the perspiration is obscure or unknown are of great importance, because an individual who is affected with fetid perspiration, while suffering from no actual bodily disease, is tormented by an infirmity which, in many cases, makes him so unpleasant to society that he lives in practical banishment. In these cases the fetid secretion is more usually confined to certain regions of the body, so that the term bromidrosis localis has been given to this form of odorous perspiration. This complaint usually affects adults of fair complexion and sanguineous temperament, more usually of the female than of the male sex; it is chronic and rebellious to treatment, not necessarily accompanied by profuse secretion from the skin, nor obviously connected with any particular kind of food or special state of health, excepting sometimes with deranged menstruation. In many cases the most strict attention to cleanliness and the greatest care of the skin seem to have little influence upon it. In women it is said to be cured permanently by marriage. It was well known by the ancients; and to this complaint Paulus Egineta referred in his prescriptions "for fetid smell and sweating at the armpits" (Book III., Sec. XXXVI.). The Greek physicians considered that an indulgence in figs was one of its causes; Eustatius, the commentator on Homer, makes mention of two sophists, called Anchimolus and Mochus, who lived solely upon figs for food, and water for drink; he adds that their perspiration was so fetid that when in the bath everybody shunned them (*Ad Iliad*, XIII., 6). The parts of the body generally the seats of bromidrosis localis are the axillæ, the pubes, perineum, and feet; it may occur about the feet alone and not elsewhere, and though nearly always partial may affect the entire cutaneous surface. The parts usually affected with fetid perspiration have some conditions in common which throw light upon its pathology and indicate treatment; they are parts in which the sudoriparous glands are numerous and large, where the temperature is continually high, where the secretion of the skin is confined and evaporation prevented, and, with the exception of the feet, where the presence of hair follicles involves the presence of sebaceous glands. There are reasons for believing that the sebaceous glands play an important

part in the production of this smell, one of which is that where they are absent, as on the palm of the hand, fetid perspiration is not observed, notwithstanding the common occurrence of profuse sweatings from that part of the body, upon which, according to Krause, the sweat glands are more numerous than anywhere else. His calculations give from 400 to 500 glands on a square inch of the skin upon the dorsal surface of the trunk, on the cheeks, and the first two segments of the lower extremities; 924 to 1,090 on the anterior aspect of the trunk and neck, on the forehead, the fore-arm, and on the back of the hand and foot; 2,685 on the sole of the foot, and on the palm of the hand 2,736 (Kölliker, *Man. Micros. Anat.*, p. 125). Another cause for the freedom of the hands from this complaint is that evaporation from them is unimpeded. The confinement of the secretion of the skin by boots, shoes, and other coverings for the feet is the real cause of its ill odour in that quarter. Hebra remarks that persons who go bare-foot or wear but a light covering for the feet, and change it often, will have little trouble from "fetid foot-sweat," and that hence this seldom occurs in the female sex, although the perspiration is more copious in women. The odour in bromidrosis localis seems frequently due to a local decomposition of the perspiration, or of the united secretions of the sudoriparous and sebaceous glands on the surface of the body, the predisposing cause of the decomposition being some primarily unhealthy state of the blood, and the actually exciting cause retention of secretion in places where the temperature is high. Those who regard the sebaceous glands and their secretion as most at fault explain the immunity of the hands from fetid perspiration by the total absence of these glands in those parts, whereas they are large and numerous in the axilla and about the pubes and perineum, where developed contemporaneously with the hair follicles, they are constantly secreting a colourless fat, semi-fluid at the temperature of the body, designed to prevent chafing and maceration of the skin in those parts of the body. While the ordinary sweat glands contain a clear transparent fluid, without structural constituents, those of the axilla, whose tubes also possess muscular walls, are rich with cells containing fat and protein, so different from the contents of the others that Kölliker is inclined to exclude the axillary sweat glands from the class of sudoriparous glands, and to regard their secretion as peculiar (*op. cit.*, p. 127). Horner also considers these large axillary glands to be specially concerned in the elimination of the odoriferous secretion of that

region, and is confirmed in his opinion by finding them of remarkable size in this situation in the negro. It has frequently been observed that light-haired persons of either sex, but especially women, are more subject to fetid perspiration than those of a darker type; and in connexion with this circumstance the remark of Hecker is interesting: that those nations were visited by the sweating fever which are characterized by a fair skin, blue eyes, and light hair—the marks of the German race; he considered himself entitled to assume that these peculiarities in the structure of the bodies of the northern nations rendered them susceptible of the disease of which he was the historian, that they caused a proneness to fluxes of all kinds, and made sweating diseases endemic in the north of Europe, while the dark-haired southern nations, and the blacks in topical climates, remain, under similar circumstances, more free from them. Yet there are dark-haired persons who suffer from fetid perspiration almost to the same degree as do the light-haired; and the axillary odour of the negro is strong and distinctive.

The treatment of fetid perspiration, when not due to the use of food or medicines, known to affect the secretion of the skin in this peculiar manner, should be both internal and external. The sudden arrest of the secretion of the skin, which is sometimes the consequence of violent measures to check bromidrosis, has been followed by neuralgia and other constitutional disturbances, whence the complaint was formerly regarded as an endeavour on the part of nature to eliminate, per cutem, a *materia peccans* with whose excretion no therapeutical interference was allowable. Like fetor of the breath, it can sometimes be traced to derangement of the stomach and digestion, which having been remedied, perspiration will return to its natural condition, but the causes being generally subtle and various the treatment will differ with each individual case. Among the remedies most successful is arsenic, highly spoken of by Milton, who also recommends that, in the oldest cases, the hair of the arm-pits should be cut short or pulled out, and these parts, with the folds of the groins and the feet, to be washed every day with soap and hot water, and then dusted, after being dried, with rice powder; if any smell remains after this, the free use of chloride of zinc or permanganate of potash in lotion should be resorted to. Small pads stuffed with animal charcoal and secured in the arm-pit absorb and deodorize the secretions of this region; and arrangements of the inner clothing, adapted to the exigencies

of the case, should be made with the view of facilitating evaporation, as the more confined the perspiration the more concentrated and powerful is the odour. M. Stanislaus Martin (*Bull. de Therap.*, T. 65, p. 143) has contrived a mode of applying charcoal as a local deodorizer in fetid perspiration of the feet. A paste composed of forty parts of powdered charcoal, forty of water, and fifteen of gum should be thickly spread over a peice of filtering paper, flannel, or felt, stretched over a board or pasteboard; the paste is then covered over with another piece of paper, which is to be smoothed with the hand so as to remove all asperities; the whole is submitted to compression during an hour, after which the water is to be allowed to evaporate; when quite dry the sole may be cut out of the required size; the soles, being inexpensive, can be changed once or twice a day if necessary. Gaffard, of Aurillac, recommends the use of a lotion composed of fifteen grains of red oxide of lead and seven and one-half drachms of solution of acetate of lead; his directions are, to pound the oxide of lead in a porcelain mortar, add the acetate by degrees, and keep in a phial, and to shake the bottle whenever the remedy is used. In most cases it is sufficient to apply a few drops once a week to the parts affected. It will be seen that in three of the prescriptions for this complaint, given by Paulus Egineta, preparations of lead were used:—*R.* 1. Of liquid alum, two parts; of myrrh, one part—dissolve in wine, and use. *R.* 2. Plunge heated molybdena (oxide of lead) into fragrant wine, triturate with the wine, adding a little myrrh until it becomes of the thickness of the sordes in baths; then use. *R.* 3. Of litharge, *dr.* xv.; of myrrh, *dr.* iii.; of ammom (cardamoms), *dr.* i.—mix with wine. *R.* 4. Of liquid alum, *dr.* viii.; of ammom, of myrrh, and of spikenard—of each, *dr.* iv.; triturate with wine, and use. The commentator remarks: all the authorities concur in recommending for the cure of this complaint a combination of astringents and aromatics; they, therefore, direct us to mix alum with storax, myrrh, and the like (Paul. Eg., Book 3, Sect., XXXVI.). To render the perspiration fragrant was one of the toilet duties of the upper classes in Greece; it was generally done by the use of an ointment, in which were mixed the leaves of the cypress pounded dry, and the bark of the pine. That the breath also might be very agreeable, Paulus Egineta says:—"One ought also to remember, in the morning, immediately after being dressed, to taste a small quantity of cassia or savin."

ART. XVIII.—*Cases Illustrative of the Usefulness of the Laryngoscope in the Diagnosis and Treatment of Disease.* By ROBERT M'DONNELL, M.D.; F.R.S.; Surgeon to Jervis-street Hospital, and Lecturer on Surgery in the Carmichael School of Medicine.

THE following cases will be read with some interest by those acquainted with the use of the laryngoscope; they will also serve to show, to those who are not, that this instrument is one of real value, not only in the diagnosis but in the treatment of laryngeal disease. Although no doubt there are now a good many persons who have taken the pains to make themselves familiar with the manipulation of the laryngoscope, there are still many who look upon its usefulness as at least dubious. A friend lately told me, that on asking a distinguished Parisian physician his opinion of it, he replied, with a shrug of his shoulders, that he considered it to be a very ingenious, scientific toy. This opinion will best be combated by detailing some cases which speak for themselves. Contrivances like the laryngoscope, endoscope, &c., which tend to advance our knowledge of early pathological change, to render diagnosis more certain and treatment more successful, are indeed ingenious and scientific, but deserve some other appellation than that of a toy. The cases which I now briefly detail will, I hope, be of use in removing any such impression:—

CASE I.—*Peculiar Contracted State of the Glottis.*—Mary M., thirty-four years of age, was admitted to Jervis-street Hospital on 14th of October, 1864. She had been brought under my notice by Dr. Byrne, who had met with her among the patients at the Canal-street Dispensary.

The day before her admission to hospital Dr. Byrne examined the condition of the larynx along with me.

The patient suffered from great difficulty of breathing. This she said had come on gradually during the previous two months, but had not been distressing until within the last five days. Her voice was weak but distinct, and although feeble yet not of a whispering character. One was at once struck by the disproportion which existed between the symptoms. The dyspnea was extreme, while the aphonia was comparatively slight. Upon looking into the throat the traces of former ulcerations were visible on the tonsils and soft palate.

Upon inquiry she stated that five years before she had married a soldier, who had infected her with syphilis and deserted her. She had been treated with mercury. She had on the limbs remains of rupia; one ulcer of this kind on the leg was still in existence.

I confess that in this case I expected that a laryngoscopic examination would reveal syphilitic ulceration about the rima, but there was nothing of the kind. The rima glottidis was seen to be contracted in a most peculiar way. The superior vocal cords were drawn together so as nearly to obscure the true cords, the edges of which only were visible, at least in front. The true cords were neither reddened nor thickened. They were, in fact, so far as they could be seen, quite natural in appearance; they were rigidly fixed in apposition with each other almost in their entire length; posteriorly, near the arytenoid cartilages, there was a small triangular opening (the base towards the cartilages) not bigger than a quarter of an inch of the point of a pen. In attempting to draw in full respiration the rima did not seem to open beyond this extent; in producing vocal sounds this opening was closed, and the air passed between the vocal cords as usual.

In this case there was no ulceration; the inferior vocal cords were healthy, but some infiltration of the surrounding structures had contracted the rima glottidis, and impeded the action of the muscles moving the arytenoid cartilages. The history of the case pointed to a syphilitic origin for this affection.

The patient was ordered Plummer's pill, gr. v., every night; iodide of potassium and two small blisters, one on each side of the thyroid cartilage.

The noise which this patient produced by her breathing while asleep was indeed alarming, and so loud that it prevented the other patients in the ward from sleeping. She had no difficulty in swallowing, and, in spite of the dyspnea, bore with great ease laryngoscopic examination.

Before many days she was much better. The urgency of her symptoms had passed in a week, and she left hospital still continuing the same treatment.

CASE II.—*In which one of the Arytenoid Cartilages having become Necrosed, was Removed.*—Patrick Kerr, a man about fifty years of age, had been under my observation for some years. He had first consulted, in 1860, on account of his suffering from fistula

in ano. He had subsequently several severe attacks of hemoptysis, and pulmonary phthisis followed.

In the Autumn of 1864 he lost his voice; and when I at that time examined him with the laryngoscope I could see an ulceration which, having commenced within the larynx, extended upwards over the left vocal cord.

From time to time I examined this patient, who became very tolerant of the instrument.

On the 17th of March, 1865, he came to me in extreme distress; he seemed to be dying, so great was the difficulty of breathing, and it was with some trouble that I could understand what he said. He told me that during the night before, something had, while he was coughing, got into his windpipe; he could feel it pricking him when he attempted to swallow, and he at first thought that it was a loose tooth which had fallen out, and become lodged there.

When I examined him with the laryngoscope (which, owing to the urgency of his symptoms, could not have been done, I fancy, if the patient had not been already accustomed to the use of the instrument) I saw a yellowish body about the size of a small horse bean bobbing up and down in the rima. I had the great good fortune to catch it in the laryngeal forceps at the first attempt, and I removed it, giving the patient instantaneous and complete relief. The body which was thus removed, and which I exhibited at a late meeting of the Pathological Society, proved to be the ossified remains of one of the arytenoid cartilages which had become necrosed; it had been still retained in its place by some shreds of ligament attached to its base; and had it not been removed in the manner described would probably have caused the patient's death.

Four months later (July 29th) this patient died, but I was not able to obtain permission to examine the larynx.

It is worthy of remark, however, that his voice, although not capable of modulation, and of a whispering character, was very distinctly audible and easily understood, even after the loss of the arytenoid cartilage.

CASE III. — *Mucous Cyst near the base of the Epiglottis.*—James Connors, a young man twenty-four years of age, was admitted to the Hospital of the Mountjoy Convict Prison on January 19th, 1866. He had on the lower lip, at the right side, a cyst about the size of a cherry, which caused the lip to project, although the tumour itself was prominent on the mucous surface of the lip. This

cyst had, he said, been opened once before by a surgeon, but had returned, and he was admitted to hospital in the hope that by the introduction of a seton it might be permanently cured.

While in hospital he drew attention to something which he said he could feel with his finger in his throat, and which for the last couple of months had caused some difficulty in swallowing solids.

Nothing could be seen on simply looking into the mouth, but with the finger I could feel a soft, elastic swelling at one side of the epiglottis. The patient had no hoarseness or loss of voice, and could swallow fluids without difficulty.

With the aid of the laryngoscope I could easily see the tumour which the finger had felt. It was apparently a mucous cyst, much resembling that on the lower lip, about the size of a filbert, and close to the left side of the epiglottis—at the anterior part of aryteno-epiglottidean fold. There was no œdema of the surrounding parts. The vocal cords were healthy.

Having passed a small seton through the cyst on the lip, I, some days later, opened the other cyst with a curved bistoury, guarded to near the point with sticking plaster. The fluid which escaped was as clear as saliva, and about the same consistence. As he spat it out it was, of course, mixed with a little blood, but there was no pus present.

Having previously tipped the end of the stilet of a catheter with nitrate of silver, melted in a spirit lamp, and curved it suitably, with the help of the mirror, I introduced the caustic into the opening already made in the cyst, so as to touch the inside of it freely.

It was well in a few days, and although still in hospital (being employed as wardsman), the patient is now quite free from inconvenience in swallowing.

This case somewhat resembles one reported in the *Medico-Chirurgical Transactions* (Vol. 47, p. 7), by Mr. A. E. Durham, of Guy's Hospital. In Mr. Durham's case, however, the tumour had attained a larger size, the surrounding parts were engaged, and life was threatened.

I may mention that during last Autumn, while in the country, I was asked to see a poor woman's child, which was apparently dying from closure of the glottis.

The sounds of inspiration were like those of a child in an advanced stage of croup, and the mother said that the child had been gradually getting worse for a month. Not having a laryngoscope I made an examination with my finger, and in so doing

burst, by accident, a tumour, from which the child coughed up some muco-purulent matter, and recovered as if by magic

Had the laryngoscope been used in this case, and a diagnosis accurately and scientifically arrived at, skill would have obtained the credit of what must be attributed to mere chance and luck.

CASE IV.—*Ulceration between the Cornua of the Arytenoid Cartilages*.—Jane Cullen, twenty-nine years of age, was admitted to Jervis-street Hospital on the 5th January, 1864.

She was a very healthy-looking young woman, had been a servant, but had been obliged to leave her situation on account of having lost her voice.

A month before she had caught a cold, accompanied with severe cough and hoarseness.

The loss of voice was considerable; she could speak only in a whisper.

On examination, a small ulcerated spot was seen on the mucous membrane between the arytenoid cartilages, a similar spot of ulceration, the size of a split pea, existed on the side of the epiglottis.

A curved wire (the stilet of a catheter answers well), with a bead of nitrate of silver on the end of it, is very suitable for touching such ulcers. This was accordingly done. Violent spasm for some moments followed the application to the first-mentioned ulceration. The patient was given iodide of potassium in bark, and made to inhale a strong solution of sulphate of zinc, reduced to spray by a fluid pulverizer.

She left hospital, cured, her voice being quite restored, and the ulcerations healed, on January 17th.

ART. XIX.—*Climate of the Swiss Alps and of the Peruvian Andes Compared*. By ARCHIBALD SMITH, M.D.

ON the main points enumerated by Dr. Hermann Weber, in his "Notes on the Climate of the Swiss Alps," in a meteorological, physiological, and pathological aspect, I shall offer a few brief observations in reference to the corresponding influences of elevation on the Peruvian Andes.^a

^a The author may here be allowed to say, that his observations on Dr. Weber's Notes on the Climate of the Swiss Alps, which appeared in this Journal February, 1864, were written and communicated to that gentleman without any view to publication, and it is only at his kind suggestion they are now published.

1. Says Dr. Weber:—"The temperature is lower; it decreases in proportion to the increasing elevation at the average rate of 1° centigrade, 1.8° F. for every 545 feet. The annual and monthly variations are less great on elevated places than in plains."

Upon this proposition I have to observe, that the elevation and temperature on the Peruvian Andes do not bear to each other the uniform relation they appear to hold in Switzerland. At Lima, situated little more than 500 feet above the level of the Pacific Ocean, the mean temperature of the atmosphere, which no storm or tempest ever agitates, is approximately, 72° Faht. throughout the year; its ordinary minimum Winter temperature being about 60° , and its Summer usual maximum being about 84° in the shade.

But if we ascend to the western aspect of the Eastern Cordillera, where, at the silver mines of Cerro-Paseo, the elevation by barometrical measurement, according to Rivero, is 14,207 feet; and, according to Herndon, 13,802 feet by the boiling point of water, the mean temperature during the dry season, as observed by Rivero, is 44° Faht. by day, and 35° Faht. by night; and in the wet season I myself never observed it below 36° Faht. in-doors. In course of the months July, August, and September, the meteorological variations at Cerro-Paseo are very trifling; and the daily changes are unimportant—the weather being generally dry and sunny by day, and more or less frosty by night. So far, then, the temperature at Lima, as compared with that at Cerro-Paseo, decreases with the increasing elevation, but not in a regular scale of gradation. For example, at the elevation of Surco, about 7,000 feet up the valley of Rimac, the average temperature will be little under that of Lima; and in the inter-Cordillera valley of Huanuco, at about the same elevation—viz., on the Estate of Andaguaylla, the temperature of the atmosphere, as registered by me, in the shade of an open *veranda*, was, during three consecutive years, regularly ranging between 66° and 72° Faht. Here, then, we see that the decrease of heat does not proceed uniformly with the vertical elevation, and that the rule of one degree centigrade of decrease in temperature for every 544 feet of ascent above sea level, entirely fails on the Peruvian Andes.

2. "The atmospheric pressure decreases, or the air becomes thinner with the increasing height."

Herndon, in his work entitled *Exploration of the Valley of the Amazon*, states, that on the eastern slope of the Andes, the trade winds are so dammed up by the mountains that the atmosphere is compressed, and consequently heavier than it is farther from the mountains, though at a lower elevation. The fact is worthy of remembrance in connexion with Andine climate, inasmuch as it shows that the Atlantic slope of the Peruvian Andes, thus favoured by the trade winds, is remarkable for humidity of atmosphere, and consequent exuberant vegetation; while the Pacific slope is equally remarkable for its vast extent of arid soil.

3. "The absolute amount of humidity in the air becomes probably less with the increasing elevation, but the relative amount or the degree of saturation, is, in general, greater in the lower mountainous regions."

The law, as here laid down, fails in its general application to the climate of the Peruvian Andes. From the elevation of 1,500 feet above the Pacific Ocean—to which height the sea vapours seasonally reach (especially in the months of June, July, and August)—to the elevation of about 7,000 on the western slope of the Andes, there is a zone of about 5,000 feet, of absolute aridness, on which rain never falls. Above the *rain line*, on the Pacific side, at, or about 7,000, to the line of permanent snow at or about 15,000 feet, the intervening region is partly cereal and partly pastoral. In this way, the whole coast and lower Andine hills of Peru, may be said to be rainless, as the drizzle that falls from May to October seldom forms into regular rain-drops. But, during the wet season of the Sierra—or the Alpine range, so called, above 7,000 feet—it rains there uniformly in the afternoon; and on the Cordillera it often snows heavily at night; though on the elevated pasture grounds the snow of night usually melts into floods of water and swollen rivers with the return of the morning's sun.

4. "The rapidity of evaporation is increased in the higher mountainous regions."

For full six months in every year, the irrigation and fertility of the lower valleys and coast of the western side of the Peruvian Andes, with its *quebradas*, or deep glens, depend on the rain and snow that fall on the Cordillera, and the cold, inhospitable pasture

lands generally called "puna" in Peru.^a From this source it is that the rivers of the coast swell periodically. We may, therefore, infer that rapid evaporation on the Andine heights must be confined to the dry months, when the Cordillera sun is very hot and scorching; and, consequently, not uniform in degree, but dependent on variations of season.

On the sandy coast of North Peru, the province of Piura is so excessively dry and sunny, that the Summer temperature, in the shade, rises there to 90° Faht.; and, on a computed average, it does not rain more than once in ten years. But in Piura, both the radiation and evaporation must be more rapid along a desert shore-plain than on the cold and wet plains and *puna* regions of Junin and other table lands at the elevation of 13,000 feet, and upwards, above the sea.

5 and 6. "The motion in the atmosphere is considerably greater on the mountains than in the plains."

On the coast of Peru, strong atmospheric currents are never observed; and though earthquakes are common, a thunder storm has not been seen in the neighbourhood of Lima during the lifetime of any one now living.

7. "The air of the highest regions of Switzerland is free from marsh-malaria."

At the mines of Cerro-Paseo, I have seen intermittent fevers, though they never originate there, nor on the surrounding heights; they are imported there from either the malarial valleys of the coast, or the deep and sultry inland valleys where malaria prevails. But, wherever contracted in the first instance, "*terciana*" readily recurs in an individual whom it once attacks, at all elevations, indiscriminately, as I can attest from my own personal suffering for a protracted period, on coast and mountains, when labouring under this malady, which I had first been affected with in Lima.

8. "The sky is, in the sub-Alpine regions, more frequently dull by mist and clouds than either in the plains or the higher Alpine regions."

^a This word is derived from "Pugna," now obsolete, according to the authority of the Dictionary of the Spanish Academy, and which means "regio frigoris, asperitate inhabitabilis."

During the wet, or rather the damp and misty season, on the lower maritime plains and valleys of Peru the sky is usually canopied with white flaky clouds; and the lower limit of the arid mountainous zone—which neither rain from the Sierra, nor vapour from the sea-board ever bedew or refresh—is usually in the wet season, on the coast, skirted by a wall of dense mist or fog. But the sky of the dry intermediary valley, where the magney and castor oil plant naturally flourish, is generally throughout the whole year remarkably pure, bright, and serene. On the other hand, the sky of the Cordillera range is often shrouded in the dark mantle of the gathering storm, and the thunder rolls in loud peals, and among bursting clouds, from peak to peak in fearful majesty, during the rainy season of the high Alpine regions.

9. “The degree of insolation, or exposure to the rays of the sun, is greater in elevated situations.”

On the Peruvian Andes, the highest degree of *insolation* is experienced in the dry and hot intermediary *Quebradas*, or mountain chasms, with here and there patches of cultivated land, under the *rain line*, where fever and sun stroke are sometimes its consequences.

For full one-half the year, on the open *puna* and adjacent Cordillera, we have not only monthly, but daily obscurations of the sky, and frequently very rapid transitions of snow, hail, sleet, and rain with morning sunshine: this is the rainy season. In the dry season on the Sierra, however, the sun of the Cordillera is proverbially hot and penetrating, scorching the skin, particularly of the face, when exposed to its burning rays; just as the wind of the Cordillera, during the same season, is peculiarly cutting, desiccative, and irritating to the air passages of the nose and fauces.

10 and 11. “The respiratory movements become increased in frequency and depth, with increasing elevation; and the contractions of the heart become more frequent in proportion to the elevation.”

After a quiet residence of a few days at the mines of Cerro-Paseo, the Cornishmen in the service of the Peruvian Mining Company, who were engaged there in 1826, generally got easily over the headache and sickness which attend the *seroche*, or Cordillera sickness; but the breathlessness, hurried action of the heart, and

sensation of throbbing and tension at the temples—symptoms which usually accompany a sharp attack of the *seroche*—were always readily induced by active exertion—such as climbing a steep shaft in a hurry, or attempting to run, and even to walk fast on the open plain. In its more severe and fatal examples, the *seroche* is attended with formidable hemorrhages, such as nasorrhagia, gastrorrhagia, and pneumorrhagia, &c.

With respect to the effect of high elevation simply, or *per se*, on the frequency of the pulse, when the body is at rest, and free from the influence of the Cordillera sickness, I may just notice, and not from memory, but on the authority of my Case-book, now before me, as written in Cerro-Paseo, in 1826, that in several instances of acute disease among the Cornish miners and mechanics, &c., the pulse is mentioned as being at first full, strong, and frequent (sometimes above 100), and then it gradually comes down to 80 in convalescence, and finally, to the natural standard (72) in cases of perfect recovery. Our usual practice at the mines, however, was to send chronic invalids and convalescents, especially in cases of rheumatism, chronic pulmonary disease, and hepatic congestion, with obstinate bilious vomiting, to our sanitarium, or great health-resort, the village of Huariaca, eight leagues below the mines, and on the road to Huanuco. But the old Spanish miners, when they found their general health impaired at these haunts of dissipation and gambling, used to resort to the mineral springs at Cono, and the sulphurous baths of Yanahuanca, in the subjacent valleys.

12. “The appetite becomes increased; the thirst is likewise, in general, augmented.”

At Cerro-Paseo, the appetite of our Europeans was generally vigorous; and, in after years, when resident in Lima, I always found, in sending invalids from the coast to the mountains, that their digestion improved, provided the drinking water was good and wholesome. But I have no note or recollection that *thirst*, under the ordinary bodily conditions of health and repose, was increased at high elevations; indeed, as compared to Lima, I should say that thirst was little experienced at the mines of Cerro-Paseo, where, at one time, I myself resided for a twelvemonth.

13. “The sanguification is improved.”

By removal from the debilitating climate of the capital of Peru, on the coast, to the cereal Andine valleys of equable annual climate, at the elevation of from eight and nine to eleven thousand feet above the level of the sea, the pale, anemic votary of fashionable town life is usually observed to gain flesh, and a ruddy cheek and lip, that are good outward signs of an improved sanguification.

14. "The nervous system becomes invigorated, the sleep, in general, more healthy."

I have myself sent the confirmed hypochondriac of the capital to the height of ten or eleven thousand feet on the Andes, with the most perfect success, and relief from this change of climate; and the martyr to spasmodic asthma in Lima, who can scarcely ever enjoy a perfect night's rest there, is often entirely relieved of breathlessness, and able to sleep comfortably, by changing his residence from the capital to some convenient Andine climate.

15. "The activity and energy of the muscular system became increased."

Within certain limits this proposition is true, also, on the Andes, but not absolutely. The native-born mountain Indian of Peru is a deep chested and firmly limbed little man. He is capable of very great muscular exertion, as has been proved on marches of extraordinary length and rapidity, with only toasted Indian corn for food, and the coca-leaf as a sustaining cordial. He trots, rather than walks, over ridges of the Cordillera, with perfect ease to himself. But, on the other hand, the Creole of the sea-board is not able for the pedestrian feats of the mountain Indian, on the loftier regions of the Sierra. At the elevation of nine or ten thousand feet, the native of the shores of the Pacific as well as the European, may feel his muscular energy not diminished, and perhaps increased, but as he ascends higher and approaches the snow line of the Cordillera, his muscular strength and freedom of breathing are sensibly diminished, and both fail him on any active exertion in climbing or walking, &c. Thus, the Cornish miner failed in muscular power at the mines of Paseo. He could not perform his day's work with the same ease as the native Indian, nor wield the same weight of hammer in the performance of it. But were the circumstances reversed, and the Peruvian Indian transported to

Truro, in Cornwall, with the help of all the coca in the world, he would certainly have no chance in muscular energy and power, as matched against the native Cornishman.

16. "The secretion of the skin is most likely augmented."

At high and cold elevations on the Peruvian Andes, it is found difficult to produce perspiration, medicinally, even with the aid of heavy bed-clothes. In warm and dry inland straths—such as that of the beautiful valley of Huanuco, for example—where rain only falls in passing showers, and where, for several months in every year, the night-dew is almost imperceptible, I have observed in my own person, that even during the most active exercise of running, the perspiration was so rapidly carried off the surface of the body, or absorbed by the air, that no inconvenience was felt from it. In climates of so mild and equable a character as this pointed at, it is probable that the average secretion of the skin on the Andes may be augmented.

17. "The urine appears to be not materially altered in quantity."

The climatic influence on this secretion I found to be very remarkable. Descending from the valley of Huanuco to Lima, during the dull, misty, and drizzling months of the year on the Peruvian coast, the calls of micturition were so frequent as to be most troublesome. I think the difference of temperature, in the shade, at Huanuco and in Lima at this season, might be about six degrees Faht. But in the former place, the air was dry and equable, with abundant sunshine; while on the coast the sky was dull, and the air humid, and produced a sensation of remarkable chilliness, which had the effect of throwing back the cutaneous secretion, and proportionally augmenting the secretion of urine.

ON THE PATHOLOGICAL INFLUENCES.

The type and pathology of diseases greatly vary with elevation, temperature, humidity, and aspect, &c., on the Peruvian Andes.

This subject, however, is far too extensive to be taken up in cursory notes and observations like the present. I may, nevertheless, adduce a few facts in illustration of the influence of aspect and elevation on climate, and, consequently, on physiology and pathology.

Mr. C. R. Markham, in his work entitled, *Travels in Peru and India*, in connexion with the chinchona plant, relates that the climate of Cuyo-cuyo, situated on the eastern aspect or slope of the Andes, in the Peruvian province of Carabaya, is much milder and more tropical in its vegetation than the climate of Arequipa, on the western declivity of the same mountain range; yet Cuyo-cuyo is 10,500 feet, while the city of Arequipa—which is surrounded by a rich and highly cultivated oasis in the desert—is only 7,850 feet above the Pacific. Between this city and the sea is a broad sandy desert, and behind it, towards the Cordillera side, the snow-capped “Misti,” with its overhanging mantle of blue, rises to the height of 20,300 English feet. This immense mountain is treeless, and mostly denuded of vegetation, while its arid slopes are covered with volcanic ashes. Dr. Mateo Paz Soldan observes that from the end of May to the first week in August, the thermometer (Reaumur) ascends in Arequipa from 10° to 15° , and that during the remaining months of the year it goes up to 18° , as its maximum in the shade.^a The daily transitions are sudden, and there is observed in course of the day great difference of temperature between the shady and sunny side of the same street. After sun-down the thermometer often falls 20 degrees Faht. In December, January, and February, rain falls at Arequipa, but with such marked regularity, that, according to Mateo Paz Soldan (who was himself a native of that city), it only occurs from two to five o'clock, p.m. But let us leave Arequipa, in lat. $16^{\circ} 20'$ S., and lon. $71^{\circ} 32'$ W., and cross the western Cordillera to the open sugar-growing valley of Abancay, in the department of Cusco, and in lat. $13^{\circ} 37'$ S., and lon. $72^{\circ} 30'$ W., and at the same elevation above sea-level as Arequipa, we there find a hot West-Indian climate. But, if we again leave Abancay and cross to the Atlantic side of the eastern Cordillera, we find that the relation of vegetation, heat and moisture, to elevation is totally changed from that which prevails on the western side.^b

In cases of pure *seroche*, which always depend on an over-attenuated atmosphere, the removal of the sufferer from a higher to a lower level, as from Cerro-Paseo to Huariaca, at once removes the malady. So far, then, the connexion between vertical elevation and the Cordillera sickness is quite obvious. But in another disease, the limits of which have long been assigned to a few

^a See *Geographia del Peru*. Paris, 1862.

^b See my paper on the *Geography of Diseases in the Climates of Peru*. Edin. New Phil. Journal, Vol. vii., January, 1858.

thousand feet above the sea—viz., yellow fever, as seen in Jamaica and Mexico, &c.; the late epidemics in Peru have far extended these limits, and exhibited modifications of a very remarkable kind in connexion with elevation and temperature, &c. For example, when persons infected with this fever arrived in the city of Arequipa from the seaport Islay, many of them died with black vomit; but in each separate case the germ of the disease seemed to have died with the individual. It did not propagate itself or become epidemic in Arequipa, and it has been inferred that this exemption was owing to the dryness of the locality. In the still drier climate of Piura, in N. Peru, the yellow fever epidemic was exceedingly mild; and cases of black vomit were there, as in Arequipa, confined to individuals from the seaport. But, strange to say, this fever, which first showed itself on the coast, and did not, when introduced to Arequipa, spread within the immediate environs of the city, no sooner took effect beyond its suburbs, on the road to Puno and Cuzco, than it proved most mortiferous. At the poor Indian villages, Chihuata and Poci, five leagues to the east, and south-east, of Arequipa, in a cool climate, at the elevation of from nine to ten thousand feet above the Pacific, the fever committed great havoc, and also extended *inland* to high elevations, in the department of Cuzco, &c. It is quite true, that at such high elevations on the Andes this yellow fever did not originate; but experience proves that it preserved its vitality and individuality of type in regions where the mean temperature did not fall below fifty degrees of Faht., and at an elevation of above 11,000 feet. At still higher elevations the epidemic did not cease; but it was observed to change its type more to the form of typhus, without any marked lesion, on *post mortem* inspection, being observed in the stomach or intestines—organs which were notably affected at lower Andine levels, of from 1,800 to 10,000 feet above the sea.^a

In the normal atmospheric constitution of Arequipa, catarrhal fever is one of the prevalent disorders of the locality, and it often merges into the inflammatory form. The *tabardillo* fever is one of the scourges of the population. It ordinarily sets in with inflammatory symptoms, and after eight days duration, assumes the low nervous form, with cerebral lesion and prostration of strength. Pleurisy, pneumonia, and dysentery (probably aggravated by the

^a See article on Hæmagastric Pestilence, in the new and abridged edition of Dr. James Copland's Dictionary of Practical Medicine, with its references on the Yellow Fever of the Peruvian Andes.

bad river water chiefly used by the poorer inhabitants) are, also, but too common and fatal diseases.^a

Goitre, so abundant in the warm and deep valleys of the department Curco, is not once mentioned among the diseases prevalent at Arequipa. Indeed, goitre is properly a disease of the inter-Cordillera glens and valleys of Peru; and, as for *cretinism*, I never, in a quarter of a century, met with as much as a single example of it, either on the coast or mountains of Peru. But, on strict inquiry at Lima (so late as 1859 and 1860) I learned that in the Province of Pataz,^b and in the village of the same name—where goitre is prevalent, and the temperature warm—with a population of 1,500 souls, there are some deaf, dumb, ill-shaped, ugly, and idiotic human creatures, who may be affected with cretinism. But if cases of this kind are ever seen in Peru at the elevation of the Cordillera, or, as it has been asserted, as high as 14,100 feet above the sea, it must, I think, be a mere incident of travel, and not because the disease is indigenous at the foot of the snow-line. This I have shown more fully in an article titled “Gleanings in Peru,” published in the *Edinburgh New Philosophical Journal*—New Series, 1860.

During my residence at the mines of Cerro-Paseo I never had before me but one case of apoplexy. It arose from careless exposure to insolation.

Contrary to what might at first be expected, I have known organic disease of the heart, with aneurism, quite relieved by a removal from the coast to the mountains. This case proved very oppressive, and ultimately fatal, in Lima, to a young officer who, during active military service at the elevation of ten or eleven thousand feet on the Andes, in the valleys of Tarma and Jauja, felt his breathing quite free, and was capable of taking most active exercise.

Incipient tubercular phthisis, usually attended with more or less hemoptysis, is one of the most common pulmonary affections known in Lima, and other parts of the coast of Peru. It is, besides, a disease almost certainly curable, if taken in time, by removing the coast patient so attacked, to the open inland valley of Jauja, which runs from ten to eleven thousand feet above sea-level.

^a Chicha, or Indian corn beer, is chiefly used, instead of water, by the rural population of Arequipa.

^b The Province of Pataz is full of deep and hot valleys with high hills between, and the goitre is there ascribed to the insalubrity of the spring water used by a section of the inhabitants.

This fact has been known and acted upon from time immemorial, by the native inhabitants and physicians; and I have myself sent patients from the capital to Jauja, in a very advanced stage of phthisis, with open ulceration and well marked caverns in the lungs, and seen them again, after a lapse of time, return to their homes free from fever, and with every appearance of the disease being entirely arrested. But in many such instances it would, after a protracted residence on the coast, again become necessary to return to the mountains to prevent the recurrence of the malady.

My space will not allow me here to enter on this subject, which I have elsewhere noticed more in detail.^a

I may, however, say in conclusion, that at the distance of twenty-five leagues from the town of Lambayeque, on the coast, is situated the Indian village, called Cachen, on the lowest summit of the western Cordillera, as it approaches the shore of the Pacific; and the water-shed falls equally to the east and to the west from this ridge. Its climate produces wheat, potatoes, and pastures, but no tree except the elder. In the open air at midnight, and in the dry season, the thermometer of Reaumur here fell to 4°, and rose to 7° or 8° by day within doors. In the sun the rise would probably be nearly twice as high as in the shade. Now this is one of the acknowledged health resorts for phthisis and hemoptysis contracted or developed on the coast of North Peru, and cases of recovery at this place are well accredited among trustworthy natives, who have themselves experienced its advantages. But the equable climate of the strath of Huanuco, with a night and day range of temperature from 66° to 72° Faht. all the year over, is not favourable in similar cases, as I had the opportunity of testing: but at colder elevations up the sides of this Elysian Valley, I have known decided benefit accrue to the phthisical and hemoptic patient.

I am led to offer the above notices on certain Andine climates in reference to tubercular consumption, with and *without* hemoptysis, and in the white as well as in the mixed races of various shades of colour, such as are usually sent from the coast to the mountains of Peru for restoration to health, as a response to Dr. Hermann Weber's appeal to the whole medical profession for information on the influence of the Alpine climate of Switzerland, beyond the elevation of 5,000 feet above sea level, on consumption, and on the

^a See Edin. Med. and Surg. Journal, No. 144; und also the Brit. and Foreign Med. Chir. Review, No. xxxvi., Oct. 1856.

very first stage of this disease. And though the elevation of 5,000 feet in the tropical Andine climates of Peru implies very different physical conditions, as the preceding observations sufficiently manifest, to the same height above the sea in Switzerland; yet, as regards the influence of elevation as favourable, or otherwise, to the production, or removal of, pulmonary consumption, in comparison to localities on the sea-board or lower plains, I think the general question involved in Dr. Weber's inquiry is essentially the same in its practical application on the Andes as on the Swiss Alps.

In regard to Peru, it will be proper always to bear in mind that its great sanitarium for hemoptic and phthysical patients from the capital, is the valley of Jauja, in the central department of Junin, at the distance of 120 miles E. of Lima, and beyond the western Cordillera, in lat. 11° 50' S., 75° 25' W. The breadth of this valley varies, but it may average ten or twelve miles; while its length is stated to be, little more or less, fifteen Spanish leagues, or about fifty miles. In the harvest season it is delightful to see the whole strath of this vale, to the extent of from forty to fifty square leagues of the most fertile soil, all covered, up to the base of its sloping and pastoral hills, with abundant crops of Indian corn or maize, wheat, barley, and potatoes, with intervening fields of green lucern, potatoes, and other roots. The river Jauja, which originates in the lake of Chinchaycocha, on the lofty plain of Junin, glides majestically through this valley; and on its left bank (amid numerous villages and hamlets, enlivened and beautified with trees of the most verdant foliage, scattered on both sides of the river) stand the two important towns, named Jauja and Huancayo, which are the principal health resorts for phthysical invalids from Lima. Huancayo enjoys a milder and warmer climate than Jauja, being considerably less elevated above the level of the sea than the latter. At Huancayo, 12° 0' S. lat., and 75° 12' W. lon.; the annual range of temperature in the shade may be taken as ranging from 8 or 9° to 14° Reamur; while at the cooler town of Jauja, with from 10 to 15,000 inhabitants, the range during one whole year has been observed not to exceed from 8 to 12 degrees of Reamur, or from 50 to 59 or 60 Faht.; with a sky always clear and sunny, and an atmosphere pure and bracing, which invites to out-door exercise and enjoyment. The harvest being home, the whole rural population rest from their agricultural labour for eight months in the year, which they give up entirely to amusement and feasting, trusting to the rain of heaven during the other four months in the year to

fertilize their land, and yield them more food than they require; so that they never think to extend their toil over the rest of the year by availing themselves of irrigation from the bountiful waters of the Jauja, as they might easily do. But there is no inducement; for the transport of produce to the capital would, in the present state of the roads, cost more than it was worth in the market. (See *Revista de Lima*, Oct., 1859). I think this information may be useful to the European, as showing the climate and topography of the great health resort of the consumptive in Peru. It is probable that ere long the interests of commerce, and the growing facilities of fluvial navigation, will open up the inter-Cordillera regions of Peru to the whole world. Now that a new ocean steam route is about to be opened up by the United States and Brazillian Steam Company, Para, at the mouth of the river Amazon, is to be one of the ports of departure and call—whence Brazilian and Peruvian steamers will run 2,500 miles up the Amazon. Thus, the department of Junin, with its diverse geographical zones, in the provinces of Huanuco, Cerro-Paseo, and Jauja, will, in the natural course of events, become accessible from the side of the Atlantic without the difficulties of the great western Cordillera barrier to encounter: and a health resort to all the phthisical patients of the world may be easily reached, through a graduated elevation of space and climate such as the face of our globe no where else can equal, in richness of vegetable and mineral products, within the same limits. In further evidence of the importance attached to the climate of Jauja by the physicians, government, and inhabitants of Lima, and the coast of Peru in general, I shall here briefly refer to the “*Estadistica General*,” or general statistics of the capital, as published by Dr. A. Fuentes, of Lima, in 1858. He says, on the subject:—“Jauja has always been the refuge of consumptive people, and a lengthened experience has demonstrated the favourable results of its climate.” Nevertheless, he well observes, many are disappointed of the benefits they expect from Jauja, because they do not leave Lima for that district until in the last stage of pulmonary decay; or because they do not remain in Jauja the time required by nature to insure a perfect recovery; or because, after visiting this sanitarium, instead of following a methodic manner of life suited to their state of disease, they abuse the advantages of the climate by the very help of the relief they derive from it, and commit excesses which can only lead to their premature death. But the important result, from the best procurable data, notwithstanding such instances of irregularity, is,

as stated by Dr. Fuentes, that the proportion between the *cured* and the total number of patients in all stages of pulmonary consumption sent to Jauja, amounts to $79\frac{42}{83}$ per cent. And in view of so important a general result to the patients from the capital, where the Indian soldier is singularly prone to phthisis—a disease almost unknown in his native hills—the government have of late years—in 1860—initiated a military hospital for consumptive patients from the coast, and capital in particular, in the vale of Jauja, under the direction and superintendence of Dr. Jose Cobian, himself a sufferer from incipient tubercular phthisis, and recommended by the *Medical Society* of Lima to seek his restoration to health in the valley of Jauja.—See *Lima Med. Gazette*, 15th March, 1860.

The annual rate of general mortality from all diseases among the whole population of Lima, which, by census, is reckoned at 100,000, Dr. Fuentes, on strict medical investigation, estimates at 5 per cent. And, further, from a general classification of all diseases in persons of all ages and sexes, who die annually in Lima, this most intelligent and inquiring author gives us the proportion to this total, $38\frac{1}{2}$ per cent. of fever cases; $19\frac{2}{3}$ per cent. of dysenteric cases; and the proportion of deaths from tubercular phthisis, as compared to the gross amount of deaths from all other ascertained diseases, he gives at the high rate of $22\frac{29}{30}$ per cent. This being so, we must admit—even should these statistics be allowed to be as yet imperfect—that the physicians in the capital of Peru, in common with all other ranks and classes of its rapidly increasing and variously coloured population, have ample opportunity of testing the *comparative curability* of this disease on the coast and on the mountains, at the elevation of from 5,000 to 10,000 feet, and they have fully and unanimously decided in favour of the latter, provided only the patient be sent there in the early stage of the disease.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Chloroform: its Action and Administration. A Handbook. By ARTHUR ERNEST SANSON. London: Churchill & Sons. 1865. Small 8vo, pp. 192.

SINCE the appearance of the masterly treatise upon chloroform, by the late lamented Dr. Snow, up to the appearance of the present work, we confess that we had been labouring under the impression that a new book upon this subject should be what our Continental neighbours would term *un embarras de richesses*—that all requisite information upon chloroform, its uses and abuses, *and more*, was contained in that exhaustive treatise, and that a new author, selecting chloroform for his theme, would find himself in the position of one having to tell a twice-told tale; yet have we lived to change that opinion, and to have to admit that this present production of Dr. Sansom's will find its appropriate place in our medical libraries. The great objection to Snow's work was its diffuseness, and its consequent bulk; but at the time he wrote it could not well be otherwise. Chloroform, as an anesthetic agent, was struggling for its very existence. Many were in the habit of using it, almost daily, who were completely in the dark as to its full potency either for good or evil; and we believe that with truth we may aver that, until Dr. Snow's work appeared, the bulk of our profession were divided in their opinions as to its merits—some blindly swearing by it, others as blindly decrying its use. That deaths occurred during its administration could not be gainsaid; but the reason why could not be adduced in any way approaching a satisfactory manner until Snow commenced the investigation in a truly philosophic spirit. To illustrate his views the introduction of numerous cases became essential; hence the increased bulk of his work. Now-a-days, however, that we have become more familiar with this agent—

thanks to the labours of Dr. Snow—it seemed that either a condensation, an epitome, as it were, of his *magnum opus*, would be an acceptable boon to the hard-worked practising surgeon of the present day; or else, and better still, an original treatise embodying all the great facts to be found in Snow's work, unencumbered of its experiments and its cases, and fully written up to our present state of knowledge upon this most important subject; and such a work, we advisedly say it, is that now before us for consideration. On some minor details, 'tis true, we might feel it our duty to differ from Dr. Sansom; but, taken as a whole, we can but express our opinion that his work is admirably suited for the busy student and practitioner—men who, of necessity, should know all the leading facts connected with this most important anesthetic agent, and yet have not time to devote to the perusal of a ponderous volume.

The most important point—that, in fact, upon which every question of safety connected with the administration of chloroform depends—is the per-centage of it, mixed with atmospheric air, that the patient can respire with impunity. The amount of this per-centage, in general terms, may be stated to have been given by different authorities at from 3 to 5 per cent.—the latter per-centage being that adopted by Snow; however, the most recent authorities seem to approve of a state of dilution of about $3\frac{1}{2}$ per cent. of chloroform mixed with atmospheric air as being most consistent with the patient's safety. Vitally mixed up, however, with this subject is another, which at first sight might not strike the operator's mind, and yet which it is all-important, especially in the but too prevalent way of administering of chloroform—poured out at haphazard on a napkin—that we should weigh well and estimate to its fullest extent—that is, the amount of chloroform vapour that the atmospheric air is able to take up and retain in solution: a question that becomes of still greater importance when it is known that the amount varies with the temperature of the surrounding atmosphere—the higher the temperature the greater being the amount of chloroform that the air is capable of absorbing. This question is so fully expressed in the tables in Dr. Snow's work that we hesitate not to reproduce them.

“The following table shows the result of experiments I made to determine the quantity of vapour of chloroform that 100 cubic inches of air will take up, and retain in solution, at various temperatures.

Temp. Fah.								Cubic Inches.
40°	-	-	-	-	-	-	-	7
45	-	-	-	-	-	-	-	8
50	-	-	-	-	-	-	-	9
55	-	-	-	-	-	-	-	11
60	-	-	-	-	-	-	-	14
65	-	-	-	-	-	-	-	19
70	-	-	-	-	-	-	-	24
75	-	-	-	-	-	-	-	29
80	-	-	-	-	-	-	-	36
85	-	-	-	-	-	-	-	44
90	-	-	-	-	-	-	-	55

“In the above table, the air is a constant quantity of 100 cubic inches which becomes expanded to 107, and so on; but it may be convenient to be able to view at a glance the quantity of vapour in 100 cubic inches of the saturated mixture of vapour and air, at different temperatures, and in the table which follows the figures are so arranged as to show this.

Temp. Fah.			Air			Vapour
40°	-	-	94	-	-	6
45	-	-	93	-	-	7
50	-	-	92	-	-	8
55	-	-	90	-	-	10
60	-	-	88	-	-	12
65	-	-	85	-	-	15
70	-	-	81	-	-	19
75	-	-	78	-	-	22
80	-	-	74	-	-	26
85	-	-	70	-	-	30
90	-	-	65	-	-	35

“As the effects of chloroform when inhaled depend entirely on the quantity of vapour present in the air which the patient breathes, the effect of temperature on its volatility is of great practical importance. The air, it is true, does not become fully saturated with vapour during the process of inhalation, but the effects of temperature are relatively as great. If, for instance, a person inhales chloroform from a handkerchief or an inhaler, in such a way that the air he breathes shall be half-saturated with the vapour, then supposing the temperature of the apartment, the handkerchief,

etc., to be 50° , the air he breathes will contain 4 per cent.; but if the temperature be 70° , the air will contain 9.5 per cent. of the vapour."

Now let us realize this fact, presuming that the thermometer is standing, as stated by Snow, at 50° , and that the air becomes but half saturated with chloroform, the patient inhales but four per cent. of the anesthetic, a quantity perfectly compatible with safety to life; but let the same amount of chloroform be poured out on a napkin and applied to the patient's nostrils whilst the thermometer stands at 70° , he is exposed to an atmosphere, presuming, again, that the air be but *half* saturated with chloroform, containing 9.5 per cent. of the anesthetic, a percentage far beyond even the highest estimate of what can be inhaled with safety. How seldom does this most important fact enter into the calculations of the parties who take upon themselves to administer chloroform; how slightly do they consider the practical bearing of the question whether they are administering chloroform in Winter or in Summer, in a warm or in a cold apartment; and yet upon such considerations as these hangs the life or death of the patient. Considerations such as these justify every scientific chloroformist in his objections to the napkin, folded lint, towel, and all such agents, as the medium for the inhalation of the chloroform; how frequently do we not see the chloroform recklessly poured out upon some such medium and forcibly held to the patient's mouth and nostrils, no means employed even for measuring the quantity used; and can we wonder at the occurrence occasionally of fatal accidents, or rather should we not be surprised that such are not far more frequent? At first the patient's repugnance to inhale a highly saturated atmosphere may afford him, unwittingly, some protection; but as the anesthetic progresses, even this protection is removed, and so far as the chloroformist's skill is concerned, no further safety exists for his patient than that afforded him by chance; need we wonder then that any person who has scientifically considered this question, and attempted to secure the maximum of safety during the administration of chloroform, has devised some modification of an inhaler that will secure for us this most desirable object. By the folded napkin we have no means at our disposal for regulating the due dilution with air of the chloroform vapour, whilst the ordinary method of dropping the chloroform from the mouth of the bottle containing it, is in itself open to grave objections. So much depends on the

size of the mouth of the bottle and on the operator's steadiness of hand, the slightest accidental motion even on the part of a bystander being sufficient to cause an undue delivery of the anesthetic; all these considerations, we repeat, have resulted in an anxiety to devise some apparatus which would be free from these fertile sources of danger. In Dr. Sansom's work three of such apparatuses are described, namely, Snow's, Clover's, and one of his own invention, which, so far as we are enabled to judge of it from his description, appears to be an admirable apparatus, fully capable of discharging all the duties that can be required of it. In another portion of his work he incidentally alludes to Dr. Skinner's gauze frame work, acknowledging its merits, so far as its power of effecting a sufficient admixture of air with the chloroform vapour, but seeming to doubt its capacity for producing an atmosphere sufficiently concentrated for producing a profound narcosis, in fact, a fault the very opposite to that but too justly alleged against the use of the napkin. In his allusion to Dr. Skinner's apparatus, Dr. Sansom, we are sure perfectly unintentionally, seems scarcely to have done that gentleman justice, having omitted in his description a most important portion of the apparatus, the supply bottle, from which by no ordinary casualty can an over-dose of the chloroform be delivered. And as to the practicability of producing deep narcosis by its agency, all we have to remark is that repeatedly have we used it in even the severest surgical operations, ovariectomy, amputations, &c., and on every occasion it has more than fulfilled our most sanguine expectations.

To allow our readers to judge of our author's *style*, no unimportant element in any work, we shall extract the following passages, in which is contained much sound advice, first, as to the administration of the chloroform; second, as to the best mode of treating dangerous symptoms arising during its administration; and we shall conclude our remarks on Dr. Sansom's work by cordially recommending it as a safe guide on the subject of which it treats:—

“GENERAL RULES.—I. Examine the condition of the patient's health. I have said how few things preclude the administration; but every case requires care, and every case may require a treatment special and peculiar. A hasty administration, regardless of coincident circumstances, may be safe in the great majority of cases; but, obviously, the more we know about the patient the better. It is better, not only for the purpose of excluding certain cases wherein organic diseases would prove

predisposing causes of danger, but also for the purpose of regulating the steps of the administration. For example, one should, in those of feeble heart, be especially careful to administer dilute atmospheres; in those of impeded respiration, any sign of mal-aëration should be watched. The examination is of especial importance when chloroform is administered for the performance of such slight operations as tooth-extractions, simple incisions, &c.

“II. Be careful that the patient has not had food for about four hours. If in any case it is necessary to relax this rule, take especial care to avoid haste in administration, and if vomiting be induced, encourage it before proceeding further. The influence of the progress of digestion in enhancing the danger of chloroform has been before noticed. I have always thought that a tablespoonful of brandy in water, or a couple of glasses of wine (sherry or champagne) have been of service. It may be that the slight stimulus has dissipated some of the mental apprehension, but I have thought that the value has been something more. In animals I have found, not only that alcohol has diminished the tendency of chloroform to prove fatal, but it has excited the circulation when feeble.

“III. Regulate the position of the patient. The best is the recumbent, but in cases of dentistry and of certain operations about the face there is a necessity for the sitting posture to be observed. The rule should be, let the patient observe the recumbent posture unless the exigencies of the operation point otherwise. The tendency to syncope is greater in the erect and sitting position than it is when the body is horizontal. The pulse in the latter condition is more slow and quiet; thus, though it may be seventy in the former position, it will frequently sink to sixty-five, or even sixty, in the latter. The reason, therefore, that the recumbent posture is to be preferred is that the circulation is more steady and the tendency to faintness is less.

“Whatever be the posture, the rule of Dr. Richardson is most valuable. Let the patient retain the selected position for three or four minutes before inhalation.

“Remove the neck-cloth and any clothing which interferes with the free play of the chest in respiration.

“IV. Reassure the patient. I have sufficiently pointed out the necessity for overcoming fear. ‘A few gentle words or a brief conversation, enticing the mind to the consideration of other subjects than danger, or a chance of danger, is invariably a wise and safe policy.’

“V. Commence the inhalation gradually. Use no force if it can possibly be avoided. Persuasion is better than restraint. The value of the gradually progressive administration has been sufficiently shown in former pages. Recollect that any sudden increase in the strength of the vapour can produce sudden syncope.

“The administrator should observe the countenance, the respiration,

and the pulse. There has been too much dispute as to which should receive most attention. Some writers have urged attention to the pulse to the exclusion of the breathing, others *vice versâ*. The truth is, to my mind, that all the signs should receive attention—none more than the others.

“The pulse of itself gives no indication of the stage of the narcosis, but any feebleness or hesitation in it is a sign that the administration should be intermitted.

“The administrator should, it has been advised, keep one hand free for careful observation of the pulse. My own practice is, as I hold the inhaler in one hand, to keep the other upon a branch of the temporal artery. It is easily felt, and thus the pulse is constantly under observation.

“VI. Continue the administration until the necessary stage of narcosis is arrived at. These stages I have divided into three. In the *first stage*—SOPOR (perversion of sensibility) there is no distinct sign of demarcation, but it is measured by the patient's own expression. There is apparently a light sleep, a dreamy half-consciousness, frequently a knowledge of what is being done, and yet an almost complete loss of sensation. For simple incisions, the removal of a single tooth, division of tendons, in cases wherein pain is to be relieved, neuralgia, dysmenorrhœa, and even in natural labour, this stage need not be overpassed.

“In the *second stage*—STUPOR (abolition of sensibility)—there is absence of all consciousness, and a state of quietude. The signs which indicate that this stage has arrived are—(1) The occurrence and subsidence of an involuntary tremor of the muscles in the course of the administration. Usually this is the less violent the more gradually the vapour is administered. (2) The absence of winking when the eyelid is lifted up and the edge of it is touched. (3) The absence of any expression of pain when the skin is pinched or pricked. I believe that the first of these signs is the most reliable, but they should be taken conjointly. This step is the one which it is usually necessary to induce for the performance of a surgical operation.

“In the *third stage*—STERTOR (muscular relaxation)—the signs are snoring respiration and complete flaccidity of muscles. A limb, being lifted, gives no sense of resistance; usually the pupils are dilated. This stage is rendered necessary when dislocations have to be reduced, when relaxation of the muscles is rendered necessary; in operations for hernia, in certain operations in parts which are long in parting with their sensibility (the perinæum, for example), &c.

“Whatever may be the stage rendered necessary, the inhalation should be steadily persisted in till this is arrived at. Only if in the course of the administration the patient makes two or three deep inspirations in succession, the chloroform should be temporarily removed. If stertor

occur, the inhalation should be suspended; and in passing from the second stage to the third an endeavour should be made to intermit it just before the occurrence of stertor, for the symptoms of anæsthesia become more profound for a short time after the inhalation is suspended. The residual amount of chloroform in the lungs, as was shown by Dr. Snow and Prof. Sédillot, becomes absorbed, and thus deepens the existing narcotism.

“VII. When the necessary stage is arrived at allow the patient to breath pure air. If the operation be of short duration no reapplication of the chloroform will be necessary. Usually it has to be repeated at intervals. The proportion last breathed can be then again administered without any more free dilution. Again, there may be a few inspirations of air, and then a reapplication at intervals of thirty or forty seconds. Thus the necessary stage can be kept up for any length of time.

“VIII. Do not be too anxious to reapply the chloroform towards the close of the operation, for frequently there will be a return to partial consciousness, although there remains a complete absence of pain.

“IX. During the time of recovery from the effects of chloroform let the patient be as little disturbed as possible. A natural sleep is the best of sequelæ.

“If vomiting occur it is better at first not to check it. If it become persistent a little brandy and soda-water constitute the best remedy.

“Hysteria, epilepsy, and a sort of catalepsy, have, in a few instances, occurred; these are best treated as ordinary cases; indeed, they may be considered as indirect effects, not due to the chloroform.

“PRACTICAL REMARKS.—If in the course of the inhalation you notice a sign of danger, if sudden pallor occurs, if the pulse fails, if after severe muscular excitement there is sudden collapse, or if there is an evident embarrassment of respiration, at once remove the chloroform, and—

“I. Bring the patient to the recumbent position. The blood regurgitating from the system to the heart may induce in the latter renewed contractions.

“II. With the finger, or with a pair of forceps, draw forwards the tongue.

“III. Make a few alternate pressures by both hands upon the lower part of the sternum.

“IV. *Commence Artificial Respiration.*—Having first brought the patient's arms to the sides, and exerted pressure against the walls of the chest to expel some of the air, lift the arms straight above the head, then bring them again to the sides and compress. Repeat this frequently, but be sure that it is done thoroughly, the arms well extended, and the chest firmly pressed. It may be well to let another press the lower part of the sternum so as to favour expiration.

“Or, if Dr. Marshall Hall's plan of treatment be preferred—

“Turn the body, from the position in which it lies, upon the side, or rather upon the sternum. This compression of the thorax first causes an expiration which forces the residual (chloroformed) air from the lungs.

“[The efforts at resuscitation may be continued by mouth-to-mouth inflation of the lungs; but this, to a certain extent, is subservient to the muscular power of the chest of the inflator. Postural methods can succeed as well.]

“Continue the alternate movement of the body from the posture on the back to that on the sternum twenty times in the minute.

“V. At the same time let *warmth* be applied to the body. Let no cold air circulate near; do not dash cold water upon the chest.

“Let friction be employed, the direction being from the toes upwards.

“If there be a possibility, let a galvanic apparatus be sent for.

“VI. If the apparatus is at hand, place the conductor (covered with wet cloth), which is in contact with the negative pole of the primary wire of the battery, over the phrenic nerve on the right side of the neck, pressing it well in; the other conductor, also wetted, should be pressed into the epigastrium; now set the battery in action for two or three seconds—this will cause instantaneous contraction of the diaphragm—remove either of the conductors for ten or fifteen seconds, and repeat.

“VII. If after five to ten minutes there is no recovery, or if the symptoms indicating danger have been characterized by difficult respiration or coma, perform tracheotomy, but continue your efforts at mechanical resuscitation. Do not relax the efforts, even if no sign of life return, for at the least half an hour.

“VIII. Enemata of brandy and water may be administered during the process; and if the patient recover sufficiently to swallow, a little stimulant may be at once given.”

Manual of Materia Medica and Therapeutics. Being an Abridgment of the late Dr. Pereira's Elements of Materia Medica, arranged in conformity with the British Pharmacopœia, and Adapted to the Use of Medical Practitioners, Chemists, and Druggists, Medical and Pharmaceutical Students, &c. By FREDERIC JOHN FARRE, M.D., Cantab. F.L.S.; Fellow of the Royal College of Physicians of London; Honorary Member of the Pharmaceutical Society of Great Britain; Examiner in Materia Medica and Pharmacy to the University of London; London Editor of the *British Pharmacopœia*; Senior Physician to St. Bartholomew's Hospital, and Lecturer on Materia Medica in St. Bartholomew's College. Assisted by ROBERT BENTLEY, M.R.C.S., F.L.S.; Honorary

Fellow of King's College, London; Professor of Botany in King's College, and Professor of Materia Medica and Botany to the Pharmaceutical Society of Great Britain: and by ROBERT WARINGTON, F.R.S., F.C.S.; Chemical Operator to the Society of Apothecaries; and Vice-President of the Chemical Society. London: Longmans, Green, and Co., 1865. 8vo, pp. 614, with numerous woodcuts.

THE great work of Pereira must long continue to be a standard authority in materia medica and therapeutics, as it must ever be a memorial, "aere perennius," of the talent, learning, and industry of its lamented author. But its very completeness as a book of reference on the subjects of which it treats renders it both too voluminous and too costly for the majority of readers; and Dr. Farre has conferred on both students and practitioners a real boon in presenting in a comprehensive form, and within the limits of a moderate octavo volume, the more important and more practical portions of his predecessor's great work. That Dr. Farre has spared no endeavour to perform his task in every department in the most perfect manner, may be already inferred from the fact of his having associated with himself in the work, the two distinguished gentlemen whose names appear with his own upon the title page.

To effect this condensation it has been necessary, as Dr. Farre, in his preface, informs us:—1. To omit all remedial agents, except those which the author termed pharmacological—such as mental, physical, but imponderable, and hygienic remedies, or to be more specific, the influence of the mind, of light, heat, electricity, food, exercise, climate, &c. 2. To omit all pharmacological remedies which are not officinal, or contained in the *British Pharmacopæia*. 3. To omit all classifications of medicines except the two which the author himself adopted—one founded on the chemical arrangement of the inorganic bodies, and on the botanical and zoological classifications of the plants and animals which yield the organic bodies; the other founded on their physiological effects. Some other modifications, rendered necessary by the lapse of time and by the progress of science, have, of course, also been made.

Having thus briefly shown, chiefly in the author's words, how the present work differs from its great prototype, we shall now proceed to glance more particularly at its arrangement and contents.

As to the arrangement, the first of the two classifications above

mentioned confer upon the author's table of contents the quality of giving more information than such tables usually convey. Thus, by looking at it, we see at a glance what officinal plants are furnished by each natural family, and what preparations of each plant exist in the Pharmacopœia. We have likewise a list of thirty-one elementary inorganic bodies, and of the preparations derived from each. Products of fermentation, products of destructive distillation, and remedies of animal origin, likewise referred to their respective classes and orders, complete the catalogue.

In speaking of weights and measures, the author suggests that a modified decimal system might have been authorised, and that some such weight as a "dec," or ten grains, and a "cent.," or one hundred grains, might have advantageously replaced the discarded scruple and drachm.

In the body of the work, the author arranges the *materia medica* in two groups—the *inorganic* and the *organic*—subdividing the former "according to the chemical relations of its members; the latter, according to their external, or, as they are called, natural history characters." The inorganic bodies used in medicine are simple or compound. Of the sixty-six simple or elementary substances at present known, thirty-one enter into the *materia medica*, &c., of the Pharmacopœia. Of these Dr. Farre commences with oxygen and hydrogen; then treating of their compound, water, in its several forms—as vapour; fluid; or solid, as ice and snow; together with its various uses in drinks, cold injections, baths; in the preparation of infusions, decoctions, solutions, &c.; its employment externally and internally in the solid form; of fomentations, of the inhalation of vapour, &c. A similar plan is followed with the other inorganic bodies.

The portion of the work treating of remedies of organic origin is copiously illustrated with well-executed woodcuts, which greatly facilitate description. The *resina podophylli* and other new medicines whose merits have secured them a place in the *British Pharmacopœia*, are fully described. The section on the Physiological Classification of Medicines is, like the rest of the work, clear and concise. At the end of the volume is a copious posological table. On the whole, the author has succeeded in condensing within a small compass a large amount of valuable information. The work is neatly brought out in a clear type, and, as we have just remarked, is fully illustrated with excellent woodcuts.

1. *Amputation at the Hip Joint.* By J. SAMPSON GAMGEE. London: J. Churchill & Sons. Pp. 33.
2. *Excision of the Scapula.* By JAMES SYME. Edinburgh: Edmonston and Douglas. Pp. 35.
3. *Resection of the Shoulder Joint.* (Reprinted from the *Army Medical Reports*, Vol. V., 1865.) By Deputy Inspector-General T. LONGMORE, Professor of Military Surgery at the Army Medical School.

WE have grouped together these three interesting contributions to operative surgery. There is much in each of them that will repay perusal; and they have this in common, that they all bear the stamp of genuine work upon them, although each in a special manner of its own.

1. Mr. Gamgee's memoir of his case of amputation at the hip joint is brought out in the highest style of art. Printed on tinted paper, illustrated with numerous photographs, it takes its place at once among the few really handsome books which emanate from the medical printing press. Its appearance alone will secure it attention when many a smaller report is tossed aside into the waste basket unheeded. But it may claim our notice on higher grounds. The case recorded is interesting in itself, and is supplemented by some sound reflections on medical and surgical statistics, and on surgical dietetics. The case was remarkable from the size of the tumour (48 inches in circumference), from its consequent great weight, and the mechanical difficulties thence arising in the performance of the operation, and from the speedy restoration of the patient to good health from the lowest stage of debility.

The size of the tumour when removed and freed from soft tissues may be judged by its length, 29 inches, and its circumference, $46\frac{1}{2}$. It was composed of imperfectly and irregularly ossified cartilage—its weight, $93\frac{1}{2}$ lbs. In these particulars it is exceeded by a case recorded in the fourth volume *Dub. Hos. Rep.*, by Sir Philip Crampton. The enormous size of this may be judged from its measuring six feet six inches in circumference. Mr. Gamgee, however, carries off the palm for his success in removing the tumour by amputation at the hip joint under circumstances which might have deterred most men without imputation of professional cowardice. We must refer our readers to the work for the details of the case and the operation.

From the remarks on Medical Statistics we cull the following as a specimen of Mr. Gamgee's style:—

“In medicine, as in other departments of knowledge, the greatest errors have been promulgated on the alleged foundation of facts. To test the value of such teaching, two inquiries have to be instituted—1stly, into the reliability of the individual facts; 2ndly, into the soundness of the method of arranging them, and reasoning upon them. The mere enunciation of a proposition, on the basis of a number of facts, is repeatedly taken as evidence of proof, but on the slightest reflection it must be evident that it is not so. Great as was the value of the aphorism in Morgagni's day—*observationes perpendendæ non numerandæ*—it is not applicable to our time; the copulative must now be substituted for the disjunctive, and the sentence then accurately expresses the order to be followed in the process of inquiry—observations of fact have to be well weighed and then counted.”

2. Mr. Syme's little book gives a graphic account of two cases in which he removed the scapula, and a third in which he removed it along with the upper extremity. The latter case succeeded, as did one of the former, and the other patient died after two months, as much, probably, of old age (70 years) as of the operation. The cases are detailed in Mr. Syme's terse, vigorous style, and the conclusions he draws from them are as follows:—

“1st. That the entire scapula, either alone or together with the arm, may be removed without much difficulty or loss of blood.

“2nd. That the wound thus inflicted may heal quickly and soundly.

“3rdly. That the arm, if preserved, may be strong and useful.”

To which we may add his conclusion of the whole matter:—“I trust that excision of the scapula will be recognized as a legitimate and established procedure of surgery.”

3. Mr. Longmore's report is well written; the subject has been previously well studied by him. It is entitled, *Remarks upon the Cases of Six Invalids Admitted during the Year 1864, at the Royal Victoria Hospital, upon whom Resection of the Shoulder Joint had been Performed, in New Zealand, for Gunshot Injuries*. The statistics which he has got upon the subject of resection of the head of the humerus are favourable to the operation so far as they go, but further observations are needed. The results of the operation are, on the whole, as favourable as those of amputation at the joint, with the further advantage of preserving an arm of more or less usefulness.

The Principles and Practice of Medicine. By JOHN PEET, M.D. 8vo, pp. 590. London: Churchill. Bombay: Thacker, Vining, and Co. 1864.

A MEDICAL work from our Indian brethren we shall always be disposed to hail with welcome. What a field is not there presented for original observation and inquiry amongst the vast, varied, and exceptionally circumstanced population of the great Indian peninsula. Again, how much have we not yet to learn of the comportment of the European constitution under the conditions which the soldier's or the civilian's life presents in the far East.

Dr. Peet's work is chiefly for students, but "may perhaps be found in some degree useful by those medical officers who are for the first time entering upon medical practice in India." It is to be regretted that writers of practical works so often think it necessary to prefix an "introduction," or a series of chapters on "diseased states of the blood," "derangements of intestines," &c., in which it is attempted to give concise views of the present condition of various branches of general pathology. The all but invariable result is a superficial enumeration of the heads of subjects embracing a very wide range of physiological, pathological, chemical, and general physical inquiry. No exact or sufficient information is conveyed to the student who approaches any of these topics for the first time; and to the educated practitioner this part of such a work is perfectly useless. Dr. Peet has sinned in this regard, but not perhaps more grievously than many of his compeers and predecessors, in the field of practical medical literature. We regret that the time thus occupied is not more profitably turned to account with the detail of the author's own observations on disease as he has seen it in India. Indeed, in the section devoted to the description of individual maladies we miss the clinical record of disease, and fail to find in Dr. Peet's rapid summary of symptoms that power of delineation which brings disease to the mind's eye of the reader in all its vitality.

We regret not to be in a position to speak more favourably of a work from the pen of an Indian medical officer, and one who, in the great Jejeebhoy Hospital, of which he is surgeon, must enjoy such ample opportunities for the study of disease.

Register of the King and Queen's College of Physicians in Ireland ; with Historical Introduction, and an Appendix containing a Roll of the Presidents and Fellows from 1654 to 1866, an Obituary of Honorary Fellows, Candidates, Licentiates, and Licentiates in Midwifery, from 1692 to 1866, and a Catalogue of Pictures, Busts, etc. Corrected to January 1st, 1866. Pp. 128. Hodges and Smith, Grafton-street.

IN the number of this Journal for August, 1865, we noticed in favourable terms the edition, for that year, of this work; and while renewing the commendation, we on that occasion were in justice bound to accord, we have at present to notice many important and interesting additions, which, in the "Historical Introduction" are most numerous. We are glad to notice (at page 16) that the number of licentiates has increased from 597 to 862, and we feel assured the next edition will have a further progress to record in this respect. We find ten pages devoted to information concerning the present working of the various departments of the College; the enactments touching "The Library" are well stated, and since the removal of the books, &c., to the present building, and the appointment of a librarian, nothing further is to be desired; a full account is given of "The Reading Room," established in 1864 for the use of the Fellows and Licentiates; and of "The Museum," which was founded in 1861, and consists of specimens of the *Materia Medica*, presented to, or purchased by the College, with a small collection of medals. "The Medical Society" instituted in 1864, a revival of the Association of the College of Physicians, founded in 1816, "for the purpose of more intimately uniting the Fellows, Honorary Fellows, and Licentiates," is also noticed; and the articles on the "School of Physic" and "Sir Patrick Dun's Hospital," contain a short but valuable account of those institutions, in the past and present time; whilst under the head of "Examinations," we find the rules and regulations in force at present; but we believe the examination in the modern languages has been abolished since the publication of this edition. As the chief additions are to be found in this portion of the work, under the above heads, we have only to add that the remaining portions of the book have undergone careful revision, and the entire work is a faithful and correct index of the King and Queen's College of Physicians. To mention each of the thirty-five articles, so well described in the "Catalogue

of Pictures, Busts, &c.," contained at the end, would far exceed our limits, we shall, therefore, in conclusion, merely recommend this edition to the profession as worthy of that consideration and support it is sure to get, and so well deserves.

The Aural Surgery of the Present Day. By DR. W. KRAMER, Berlin. With Two Tables and Nine Woodcuts. Translated by H. POWER, M.B., London. With corrections and numerous additions by the author. New Sydenham Society. 1863. Pp. 154.

Ohrenkrankheiten und Ohrenärzte in England und Deutschland. Ein Nachtrag zur Ohrenheilkunde der Gegenwart, von DR. W. KRAMER, Geheimer Sanitätsrath. Berlin: 1865. Pp. 96.

Aural Diseases and Aurists in England and Germany; being a Supplement to Aural Surgery of the Present Day. By DR. W. KRAMER.

WE confess to feeling somewhat uncertain as to how we should notice these volumes. As they have been sent to us with the special compliments of their author, we feel bound to record our opinion of them, and yet we know not from what stand-point to view them. Are they meant as genuine contributions to aural surgery, or are they meant as a strange, and in this country, a novel experiment to demonstrate to what an extent and under how distinguished a patronage the professional peculiarities of an individual can be advertised?

We have read the *Ohrenheilkunde der Gegenwart* with feelings of amazement; and we felt surprised that any respectable English society would have mispent its money, and misapplied its patronage by translating so feeble a work; but we hardly know how to characterize our feelings, on perusing the supplement to this work, in which members of our profession, of undoubted standing and character, are alluded to and spoken of as ignorant quacks and charlatans.

The first of these works, that translated under the auspices of the New Sydenham Society, has been so fully criticised by the author of *A Vindication of the Present State of Aural Surgery*, that we shall content ourselves with adopting his opinion of it, and

asserting that "instead of being, as its title would indicate, a comprehensive and intelligent survey of the present state of aural pathology, it is in reality little more than an advertisement of the author's peculiar system of treating diseases of the ear by means of his four catheters and catgut bougies. Anatomy, pathology, and scientific surgery are sneered at; every modern plan for simplifying the study of ear diseases and for enabling them to be diagnosed and treated by the members of the profession are wholly ignored, and the author's system of using four catheters and catgut bougies is made to be the *all-in-all* of aural diagnostics and therapeutics."

With reference to the supplement, we shall examine it somewhat more in detail. Through peculiar circumstances, Dr. Kramer tells us, it so happened that during 1861-64 he spent some three or four months of each year in London, following his profession as an aurist. We may tell our readers, what Dr. Kramer does not tell them, that the period of his arrival, the place of his residence, and other details were duly advertised in the daily press; and while we would be slow to measure the conduct of a Prussian surgeon by what may be considered our own refined standard of etiquette, yet we think Dr. Kramer must have been fully aware of how unprofessional such a proceeding would appear in medical eyes.

Its results, however, appear to have been favourable, and patients resorted to the Berlin aurist in numbers. It is tempting to dwell on this subject; who does not know the mighty advantage that accrues to the medical man by his having been in foreign lands, and how intensified this advantage becomes if, by some lucky chance, he either is, or becomes, altogether metamorphosed into a foreigner. One would have thought that a great and enlightened public would by this time have found out that almost every medical man, now-a-days, has had the privilege of studying in some foreign school, and that the fact has become so common that it need not excite either their admiration or surprise. Perhaps it is coming to this; but the belief in a foreign education is still strongly held, and we must be still content in this respect to class ourselves with the *artistes* of at least an equally harmonious profession, thinking ourselves fortunate that, unlike them, we are not as yet obliged to affix an Italian termination to our names.

To lead the reader, however, to imagine that Dr. Kramer had any such faith in foreign doctors would be to seriously misrepresent him. Utterly despising all the continental aurists, he has by his

visits to London been brought into such close contact with the literary and practical activity of the English, and allow us to add of the Irish aurists, that he very soon learnt to think very little of them indeed; so little that it was not enough that he should have indirectly alluded to their errors in his *Aural Surgery of the Present Day*, but that he felt compelled to publish this supplement, in which he tells us of some 300 cases of ear diseases which he had seen in England; and then proceeds to show how little every living aurist but himself knows of ear diseases, especially those of the middle ear, and how grossly ignorant they are of the use of the Eustachian catheter; not only is Toynbee in this respect a "wretched aurist," but as to Wilde, his very assertion that he uses such an instrument deserves no credence whatever (*Seine Versicherung den Katheter "in vielen Fällen anzuwenden" verdient desshalb auch gar keinen Glauben*); and so with Von Troeltsch, Erhard, Voltolini, Schwartz, Weber, Politzer, and others. It is so easy to verify these statements that even at the risk of being a little tedious we will venture to give a few select extracts from the criticisms on some English aurists, whose works will be easily accessible to the reader, and so the justice of the criticisms be the more easily comprehended. Nobody but Dr. Kramer would appear to know much, if anything, about diseases of the middle ear, and most elaborate and often times very unfair attempts to prove this are made in his supplement. According to Dr. Kramer, "Toynbee treats very superficially of chronic inflammations of the tympanic cavity; indeed so much so that his views need not be especially criticised; but while he talks of 'rigidity' of the mucous membrane of the tympanic cavity, he in his 'Descriptive Catalogue' details as the result of *post mortem* examinations only such appearances as 'swollen,' 'reddened,' and such like, but never 'rigidity.' How comes it then that Toynbee, who is so proud of the pathological-anatomical ground-work of his system of aural diseases, can treat of such a form of disease as 'rigidity of the mucous membrane of the tympanic cavity,' when he never yet proved, by his anatomical researches, that such a state of things existed; this does not, however, prevent him from stating that the diagnosis of this form of disease is by no means difficult. 'One has but to inspect the tympanum, which is often but little, if at all, transparent! sometimes even swollen!! The Eustachian tube admits the air either with its usual or very slightly altered sound.' Equally trifling are the subjective symptoms—'inability of understanding the conversation when several

people speak at the same time, or when one speaks too rapidly—or inability of appreciating the sounds, though the voice is heard; such symptoms, and the history of the disease will show that the case is one of hardening of the mucous membrane of the tympanic cavity! and when one takes into account the absence of all the symptoms indicating nervous deafness, will enable the attentive observer to form an accurate diagnosis.’ Such a conclusion makes Toynbee’s *naïveté* (not to use a stronger expression) really perfect. Of course the curing of such a disease thus so imperfectly diagnosed offers no difficulty whatever to Toynbee.” “One must be of a very easy and complacent turn of mind indeed, to indulge, like Toynbee, in such phantastical views of such hypothetical ear diseases, and to treat thus of their nature, diagnosis, and curability.” “Again, Toynbee writes of other pathological changes met with in the mucous membrane of the tympanic cavity, such as ‘membranous bands in the cavity;’ but then without treating of any attempts at diagnosis, he gives the practical surgeon the very discouraging information that ‘during the lifetime of the patient this pathological condition cannot be diagnosed from that of rigidity of the mucous membrane;’ so that when we remember that pathological anatomy has thrown no light on this latter disease as met with in the living subject, and that nothing can be done for its permanent cure, it is very obvious that quite the same must be said about this latter affection. It is therefore somewhat satisfactory that Toynbee, with his accustomed confidence, declares that the treatment of both diseases being the same, it is not of much consequence to distinguish the one from the other during life! This gentleman possesses indeed a most enviable amount of self-confidence!! For the grossest inconsistencies he does not care in the slightest!! For example, with, and in spite of the declaration that bands of membrane cannot be distinguished during life from rigidity of the mucous membrane, and thereupon cannot at all be diagnosed, he says:—‘We should take care to distinguish between bands formed by organized membrane from those consisting of dried up mucus; of course he never stops to indicate the diagnostic mark between them, but then the treatment is the same. Spanish fly-blisters, stimulating blisters, and such like, will in from four to six weeks cure everything—be it rigidity of the membrane or membranous bands, organized or consisting of but hardened mucus.’”

Anchylolysis of the stapes to the fenestra ovalis is also alluded to in detail, and an abstract of Mr. Toynbee’s statistics on this subject are

given, and criticised in the usual strain:—"While stating that the diagnosis of this form of disease causes little difficulty in the majority of cases, if one only carefully examines into its origin and progress, and properly inspects the ear with the necessary accuracy, he shows by the analysis of his five selected cases how little he acts up to his own advice; the details of these cases showing little but the 'slow development of deafness,' not an uncommon feature in many different diseases of the ear; and so far as the accurate examination of the ear itself is concerned, Toynbee has, in four cases out of the five, altogether neglected it, and in the fifth simply inspected the membrana tympani, and tried the method of Valsalva. Such trifling inconsistencies do not, of course, flutter a man like Toynbee, who goes even still further, and points, in his diagnosis, to the striking similarity which exists between the symptoms of this kind of disease and those of rigidity of the mucous membrane of the tympanum; but we saw that this purely hypothetical disease was not itself to be distinguished from membranous bands in the tympanum, so that we can take our choice as to which of the three diseases our patient labours under; not that to the practical surgeon it need make much matter, as Toynbee makes the consoling declaration that the treatment is the same in all these cases. Under such peculiarly convenient circumstances it is simply as a luxury that I enumerate here—just for curiosity sake—the special symptoms by which Toynbee, after all, teaches us to recognize this anchylosis. They are 'slow development of deafness, dryness of the auditory passages, singing in the ear, hearing better while driving, reddish swelling of the membranous auditory passages, enlarged tonsils, membrana tympani thickened or often not! etc., etc., all symptoms, that from their vagueness, and after what I have written about Toynbee's diagnosis of diseases of the middle ear, cannot be considered by any aurist who has common sense left, as characteristic of anchylosis of the stapes to the fenestra ovalis; and when, in conclusion, Toynbee points out that rigid attachment of the basis of the stapes is really capable of being much ameliorated; when he even speaks of there being an advantage in the stapes having an expanded basis, obtained through the resorption of the thrown out bony material, so that only such cases, where there is a bony coalescence between the articulation of the stapes and the vestibule, appear to him really hopeless; then, indeed, it is nothing but ridiculous charlatanery (*ganz lächerliche Charlatanerie!*). The gentleman has really no knowledge whatever of the existence of

all these conditions in the living subject, how then can he know whether they have ever been cured, or even improved; and, at all events, the preceding observations justify me in abstaining from all further examination of Toynbee's writings, and justify me in stating that while he has dissected very many ears, and made himself acquainted with their pathological conditions, yet as he has not seen these ears during life, the result of these dissections are lacking of any real connexion with the pathogeny of deafness. They are nothing but an accumulation of bare unconnected facts, and Toynbee's merit for the mere multiplication of such is very light; and hence not only does he himself remain a bad aurist (*ein schlechter Ohrenarzt*), especially with regard to diseases of the middle ear, but he has also been the means of misleading the younger German aurists into a similar superficiality in their description of diseases of this part. None, however, of the living German aurists have sunk so low as Toynbee, because none of them, except Schmalz, have neglected, as much as Toynbee has, the study of ear catheters."

"I know quite well that my opinion of this gentleman will call forth a cry of indignation from his many followers, as Erhard, V. Troeltsch, Voltolini, Politzer, and others. I, however, entreat these gentlemen, beforehand, to abstain from mere empty declamation on this subject. My opinions are based on numerous grounds, and whoever cannot confute them must take the consequences."

Perhaps the reader may by this be amply able to judge of Dr. Kramer's style; but as the next name called up for judgment is one very well known to them, we, even at the risk of taking up a little more space, venture to give a few more extracts. A good story is told of an old rector who, when called upon to preach before the members of his university, unfortunately selected a sermon long before preached in a remote country parish. The text was good, and the introductory matter not bad, and then the discourse sprouted, as such discourses generally do, into two heads; the firstly was to the young, and the college lads sat trembling; their seniors complacent awhile, while warnings, and threatenings, and denunciations were launched against them—poor unoffending souls; but the secondly came, and the wonder of all was great and the surprise terrible when the preacher read—'but as for you, you hoary-headed sinners, how shall I warn you from the wrath to come.' So Toynbee may be an ignorant boy, but Sir W. Wilde is an old offender, and it is not by accident that Dr. Kramer takes care to tell him so:

“He is the author of a work on aural surgery, but he never himself dissected a single diseased ear. In his lamentable neglect of the ear catheter and in his groundless confidence in the diagnostic value of Valsalva’s method, he, in point of time, precedes Toynbee. He reduces the examination of the symptoms of the diseases of the middle ear to recording whether the tympanum can be inflated or not, as observed by Valsalva’s method, and to the observation of the audible or *inaudible* sounds, although the latter are designated by Wilde as ‘unimportant.’” This is really good; but, however laughable, it simply shows that Dr. Kramer has not only not understood this portion of *The Aural Surgery*, but has most positively misquoted it. In justice, we feel bound to give the sentence entire:—“If the patient be able to inflate the tympanum we may apply our own ear, either directly or through the intervention of a stethoscope, and the peculiarity of sound which is thus produced, *i.e.*, by the inflation in the middle ear, either the ordinary normal rush of air or a prolonged squeeling or gurgling sound, such as might be caused by any contraction in or thickening of the walls of the Eustachian tube, or by dryness, or by the accumulation of mucus, either in it or in the cavity of the tympanum, is heard; the stethoscope should also be applied over the mastoid process, and the same series of observations made upon *the sounds, if any, produced there*; but these latter (evidently referring to those heard near the mastoid process) can seldom be heard distinctly.”

“Wilde, for courtesy sake, it is true, calls the ear catheter a valuable means of diagnosis; but he soon afterwards corrects himself, and says:—I have latterly found it very seldom necessary to resort to its use. It is therefore more than probable that Wilde never uses it now-a-days, and this is rendered the more probable because in the detailed examination of some two hundred patients ear catheterism is not once mentioned; but even in these seventy-five cases it is by Valsalva’s method that the middle ear and Eustachian tube were found uninflatable; his assertion therefore that he uses the catheter in many cases deserves no credence whatever. What may be his reason for thus neglecting this ‘valuable means of diagnosis,’ can scarcely be stated, if it be not entire want of skill on his part to use it; indeed Wilde’s thinking it quite sufficient to be provided with a single silver catheter, his talking of its being easily shifted by speaking or swallowing, and his stating that the passing of a catgut bougie through the Eustachian tube is impossible, even in a dead subject, are clear proofs that he does

not know how to handle the instrument, and is but hunting for reasons why not to use it." Many such assertions are met with. At one time we read that "Wilde's statements savour more of uncertain surmises than of full convictions;" again, "Wilde records cases which are most unsufficiently diagnosed, indeed in some instances he gives cases having no results, and altogether unworthy of mention, and this though he calls them himself 'excellent cases,' and though he be, like Toynbee, a professor and teacher of aural surgery."

"As Wilde himself tells us that it is impossible to diagnose accurately the pathological conditions of the tympanum, it would be useless to criticise his negative diagnosis of nervous deafness; but he seems to admit the insufficiency of his method of examining such cases, and in this we agree with him, believing that to him all diseases existing on the other side of the uninjured membrana tympani are but as a sealed book or as an enigma about which he knows nothing, and for which he yet applies vesications, corrosive sublimate, &c."

We have thought it right to justify the opinion we have expressed of this book by thus giving very briefly an account of how two very able and eminent surgeons, both of them well known to our readers, have been criticised and condemned by Dr. Kramer. All the other names we have above referred to are equally severely and unjustly condemned; but the instances already given will be amply sufficient to give an idea of the general tone of this supplemental volume; and in taking leave of its author, we wish to tell him that while we would be the very last to put an end to free discussion, yet that we draw a well marked distinction between criticising an author's views, as they are to be met with in his published writings, and publishing a work which hardly contains a word of true criticism from its beginning to its end; but consists in an attempt to prove that no one is capable of performing a not very difficult surgical operation but himself, and that of all aurists he is the only one that knows his business.

This supplemental volume will probably not circulate very much, at least in its present form, in this country. The *Aural Surgery of the Present Day*, on the contrary, had a very extensive circulation, having been introduced to the notice of British medical men under the auspices of the New Sydenham Society. There can be no doubt now that in thus doing the New Sydenham Society committed a very great mistake, and that it to a certain extent lowered

its standard of merit by undertaking the sponsorship for such a volume.

In catering effectively for a large number of subscribers it must always be a most difficult task to please even a large majority, it must be a hopeless one to please all, and it is far from us to say one single harsh word against a Society which already has published so much that is excellent and good; and yet we think there are very manifest signs of indisposition to be found in this Society, and that unless its energetic secretary—who is at the same time its most active and worst paid officer—does not do something to arouse it from its present unsatisfactory condition it will be very likely to soon go the way of its progenitor, the Sydenham Society of 1843-'56. In our opinion, the peculiar functions of such a medical and surgical society ought to be to keep us acquainted with all the best modern literature of the day; now it is notorious that to the greater number of our medical brethren, German medical literature is unknown, and to a less extent French literature also, and it is equally a fact that many medical memoirs published in Austria, Prussia, Bavaria, and Saxony would, if properly translated into good English, be most acceptable to our medical men. For every plate representing some skin disease we might have some recent monograph on some important subject by Hebra, or Virchow, or Langenbeck, or many such, translated and published; and there is this to be borne in mind, that such publications are not accessible at present, whereas plates of skin diseases are; those by the late Dr. Elfinger, still publishing in Vienna, can be easily obtained by those who want them; and while they may not surpass, they certainly equal, anything of the kind published by the New Sydenham. Suppose we put the case this way:—A fasciculus of plates of skin diseases gratifies one hundred subscribers, will there not be at least three hundred gratified if monographs on different subjects, of course varying in size, were published instead. As to the *Year Book*, we have always regarded it as an important publication. But the compilation of a year book must in this country always be an expensive task; so, as a matter of expediency, we are glad to see its publication is to be but biennial; and unless when the matter recorded is very remarkable or novel we trust there will be no extracts.

We wish the society every success; that it should make mistakes now and then is what, of course, we must expect; that it made one in publishing Dr. Kramer's *Year Book*, we fully believe; but we would suggest that it is another mistake not to cease the publication

of its *Atlas*, which, after all, benefits but a small section of its subscribers and a London artist or two; and we would recommend it to take upon itself more the functions of a translating society—translating, however, nothing but good works, and leave the publication of original matter or plates to other and somewhat more legitimate channels.

A Description of the Diseased Conditions of the Knee-joint which Require Amputation of the Limb, and those Conditions which are Favourable to Excision of the Joint, &c. By PETER CHARLES PRICE, late Surgeon to the Great Northern Hospital. Edited, with a Preface and Memoir of the Author, by HENRY SMITH, F.R.C.S., Assistant Surgeon to King's College Hospital. London: John Churchill and Sons, New Burlington-street. 1865.

THE posthumous works of an able man must always claim a peculiar attention. Speaking to us as it were from the grave much of the ordinary feeling with which they would otherwise be perused will of necessity disappear, and will be replaced by a special reverence. Criticism of style will not be thought of; imperfections and deficiencies will attract less remark; and the dicta, which under other circumstances would not have weighed with us, obtain an almost Delphic value. When a surgical work so circumstanced is still further commended to our notice by the exertions and affectionate sympathy of one who was not only a personal friend of the deceased author, but who has also made for himself a reputation in surgery, we feel that we are bound to give its contents more than ordinary attention. For this purpose, finding ourselves debarred from ordinary criticism, we shall endeavour to give a short analysis of the work before us, in the hope that our readers, who are interested in the important branch of conservative surgery, which forms its subject, will be induced to seek a closer acquaintance with it.

The circumstances which gave rise to this work are related in the short biography, which Mr. Henry Smith has written of its author:—

“The Royal College of Surgeons had announced for 1860-61, the following title as the subject for the Jacksonian Prize Essay:—
‘A description of the diseased conditions of the Knee which require

amputation of the limb, and those conditions which are favourable to excision of the joint; with an explanation of the relative advantages of both operations, as far as can be ascertained by cases properly authenticated.' And to use the eloquent words of his friend and biographer, Dr. Webb, 'With his wonted energy Price threw himself into the work. He sacrificed his leisure, and even his rest, to it; and he produced an essay on excision of the knee-joint, which for the fulness with which the subject is treated, the vast number of facts brought together, the beautiful preparations, drawings, and photographs which accompanied it, is unequalled in the records of conservative Surgery.'

"An immense amount of labour was bestowed upon the compilation of this Essay; it entailed not only a large extent of personal research and observation, but a large correspondence was necessary in collecting data connected with the operation, from all parts of the United Kingdom, from the Continent, and from America. Knowing, as he did, that few besides himself could have had the same opportunities of observing facts in reference to this proceeding, he looked forward with the utmost confidence to the result of the award. Those few of his friends who knew what he was about, were equally sanguine with himself. And I can truly state that it was from the knowledge that Price was writing for the prize, that there was no other competitor for the same. A very distinguished and well-known fellow pupil had already collected material for the Essay; but on its being represented to him that such a formidable adversary as Price was working for the prize, he gave up the task.

"After months of labour, both night and day, the Essay was completed, and sent into the College, accompanied by numerous illustrations and preparations. At this period, viz. 1860, the time-honoured custom of re-election to the Council—the '*imperium in imperio*'—had not been broken through, and there were but few members of this august body who practically knew much about conservative Surgery. Such men as James Paget, Henry Hancock, and William Fergusson, had not a seat there then, unfortunately for poor Price. Will it be believed that, of the three gentlemen who were appointed to adjudicate for the Prize, only one of them had ever performed the operation of excision of the knee-joint! Most of my readers will agree with me, I think, in stating that the Council of the College of Surgeons had no right to propose such a subject for the Jacksonian Essay, if they could not find amongst their body three surgeons who were capable, from practical experience, of adjudicating upon such a much-vexed question as the relative merits of curing disease of the knee-joint [*sic.*] by amputation or excision.

"The result of the deliberation of the three gentlemen appointed by the Council was, as might be pretty well anticipated under such peculiar circumstances, that the Essay was returned to its author as not worthy of their acceptance; and this, *notwithstanding that no other essay was sent in.*"

The rejection of his essay, under such circumstances, bore heavily on poor Price's health and spirits; and although his well-known industry and ability were rewarded, not long after, by the appointment of assistant surgeon to King's College Hospital, he never regained his physical strength, and in 1864 he died of pulmonary consumption. There is no doubt that by his death the surgical profession sustained a great loss in the person of one of its most gifted and promising members.

The want of a table of contents and index is very insufficiently supplied by the following analytical sketch of its subject, furnished by Mr. H. Smith in the preface:—

“In the first part the anatomy of the knee-joint is described; then is given at length a sketch of the various diseased conditions which demand the operation of amputation or excision. The history of the latter proceeding, both in this country and abroad, is next furnished. Then is given the analysis, with details of all the cases which have been performed in England, as well as of those which were operated upon in Germany, France, and America. Next follows a record of the cases which have occurred in Military Surgery. We are also furnished with an analysis of the fatal cases occurring after this operation. He makes an elaborate inquiry as to the cases to which the operation is applicable, and the circumstances under which it should be performed, and enters minutely into the objections advanced against the proceeding. In the latter part of the work will be found a consideration of these cases of disease of the knee-joint, which are to be treated by amputation, with the respective mortality after each operation; and lastly, the author sums up his own deductions, and gives us the result of his own experience in detail.”

Over part I., which contains a general description of the anatomy of the knee-joint, we need not linger.

Part II. gives a short, and on the whole, satisfactory sketch of the various diseased conditions of the structures comprising the knee-joint, which, under certain phases, demand a recourse to amputation of the thigh, or excision of the knee-joint. This part, in fact, constitutes the bulk of the volume, and will be perused with much interest.

The subdivision of this chapter are as follows:—I. Diseased conditions of the synovial membrane. In this paragraph, which deals of acute synovitis, there is a good description of the affection, but nothing specially requiring notice. The same remark applies to II. On sub-acute inflammation of the synovial membrane. III. On Chronic synovitis contains the following:—

"Swelling of a definite form is one of the earliest and most clearly-defined symptoms; while the absence of pain, and even tenderness, is often so remarkable, considering the amount of apparent mischief, that their non-existence is sometimes sufficient to deceive the surgeon and patient as to the exact state and progress of the disease. I believe that there are few surgeons who will not readily admit that pain is a most equivocal and deceptive accompaniment of joint diseases. I have over and over again been deceived as to the nature and extent of diseased action invading the knee-articulation, because I have looked upon the presence or absence of acute pain as a chief diagnostic feature; and in recording instances in which excision has been performed, it will be shown that extensive destruction has often taken place in the joint without the occurrence of any very considerable pain or annoyance."

We believe, with Mr. Price, that the presence or absence of pain is no criterion of the actual condition of a joint which has been for some time the seat of chronic synovitis; but we must add, in explanation of or addition to the above, that the amount of mischief is generally proportioned to the aggregate of pain which has been suffered from time to time, and that fresh mischief is always accompanied by fresh attacks of pain and heat in the joint: the pain may be dull and intermittent, and the heat may not be very great, but both can be detected; and if detected, generally subdued by cold or tepid bandaging, using opiate solutions to moisten the cloths, by cautious leeching, and by avoiding irritants and counter-irritants. At the same time there is no denying that repeated though trifling accesses of inflammation in these chronic cases are the rule, and tend, unless very sedulously observed and combated, to complete disorganization of the joint.

IV. On pulpy and gelatiniform degenerations of the synovial membrane recognizes the position of this particular form of disease as a fibro-gelatiniform destruction or degeneration which quickly implicates the cartilages and other component tissues of the joint. This affection is, in fact, the meeting point of destructive inflammation of a chronic character, implicating at once the synovial membrane and the cartilages; as such it is fitly placed before V. On diseased cartilages. Here we have a due acknowledgment of the researches of Toynbee, Goodsir, and Redfern into the pathological alterations of cartilage, as correcting previous errors on the subject. The following extract gives a good idea of our author's facility of condensation:—

“With regard to this so-called ulceration, Dr. Redfern clearly observes that the process in cartilage and other tissues is exactly the same, if we regard merely the actions of the essential elements of the textures, although the points of dissimilarity consist in the non-production of inflammatory products, as exudation and purulent secretions, and the absence of all painful sensations. But the process of destruction, pathologically considered, consists in a special granular degeneration of the component cartilage cells; and the shredding and loss of tissue which obtains, is due to apparent absorption and destruction of the hyaline substance, which splits up and softens into a gelatinous and finely molecular material. These changes take place in the majority of all serious diseases of the knee-joint, although the exact nature of the cell transformation varies, and instead of presenting a granular appearance exhibits a fatty or fibrous consistence. Such variations are, however, dependent on the character of the morbid actions; for while in acute and chronic inflammatory destruction of the joint, the granular change predominates, that of a fatty nature results in such instances in which the articulation, although not positively or seriously diseased, has remained unused for a considerable period. When a gouty or rheumatic taint is plainly visible in influencing joint mischief, the articular cartilages undergo a decided fibrous degeneration. The same transformation of the cartilage-cell is apparent in the cartilages of elderly people, and in such conditions in which the synovial apparatus has been more or less chronically destroyed or removed, and the articulating ends of the bones have become susceptible of pressure. To see this fibrous degeneration it is only needful to examine a portion of cartilage on which mechanical influences have acted.”

Mr. Price has also recognized the fact, that the so-called ulceration of cartilage is often the result of serious inflammation of the sub-cartilaginous lamella of bone, and of the adjacent cancellous structure; he also has been able to dissociate pain and starting of the limb from ulceration of the cartilages, and to assign to it, as its proper cause, the above-named implication of the bone. The section on diseased conditions of the articular extremities of the bones composing the knee-joint is too brief—in fact all through these sections there is a constantly recurring *desiderium*, that the author had lived to convert his intended prize essay into the mature and perfect monograph. There is material enough here on which to form the opinion that had he lived to do so the surgery of the knee-joint would have been written in better style than has yet been attempted. Favourable to resection his verdict would, probably, have always been, but we have no doubt that with more extensive experience would have come a clear and just appre-

ciation of its position as an operative proceeding, and a scientific discrimination of the cases to which it is suited. To our mind, a perfect work on the subject of resection has yet to be written.

Want of space compels us to pass over without notice many minor sections on tuberculous disease of the articulating ends of the femur and tibia, on tumours, wounds, and deformities demanding surgical interference, &c. The history of the operation is now familiar to our readers. Mr. Butcher, whose name is honourably mentioned by Mr. Smith, in common with those of Sir Wm. Fergusson, Mr. Jones of Jersey, and Mr. Price, as having made excision of the knee a legitimate proceeding in surgery, has by his repeated essays on this subject left nothing for us to extract from the book before us on this point. The statistics of the operation are, of course, more fully given by Mr. Butcher, although not in a form handy for reference. There is a slight difference in Mr. Price's tables and Mr. Butcher's as to arrangement: the latter proceeds chronologically, while the former groups all the cases of each operation together. Each method has its advantages, but neither in their present form is perfect. In practical deductions from his statistics Mr. Price shows forth as the truly philosophical surgeon. For many of these we must refer the reader to the work itself; some extracts we cannot forbear to make:—

“In the foregoing synopsis and *résumé* of 291 cases, in which the knee articulation has been excised for disease, deformity, and accident from the year 1762 to the end of (middle of December) 1860, not only in this country, but in America and on the Continent, I have endeavoured to state,—*Firstly*, the exact number of cases with which I have become acquainted; *Secondly*, to give the name of each operator, the number of times he has performed the operation, and the result of his cases, also the nature of the mischief which necessitated the interference. In this way, although not without considerable difficulty, I have contrived to deduce the various statistics which, I have every reason to believe, are as correct as can be obtained with any degree of certainty.

“Analysing still more minutely these 291 cases, we obtain some interesting information. These 291 cases have been divided among 106 different surgeons,—*i.e.*, that only 106 surgeons, during a period of 102 years, have resorted to the operation.

“But to go further. It will be found, as already stated, that these 291 cases may be divided into sections, in accordance with the time of their performance and the residence of the operators, thus:—From 1762 to 1830, seventeen cases were operated on, with ten deaths.

"Of the nine cases operated upon by Germans from 1830 to 1849, four died.

"Of the twenty-one cases operated upon by continental practitioners from 1850 to 1860, nine died; and of 238 cases operated upon by English, Irish, Scotch, and Jersey surgeons, fifty-five died; while in six cases operated upon in America none died, so far as I am aware.

"To reduce the observations to a tabular form, it will be found that—

	Number operated on	Cured	Died
From 1762 to 1830 (mixed) . .	17	7	10
From 1830 to 1849 (German) . .	9	5	4
From 1850 to 1860 (Continental) . .	21	12	9
From 1850 to 1860 (British) . .	238	183	55
From 1852 to 1860 (American, . .	6	6	0
Totals . .	291	213	78

"It has already been stated, somewhat in detail, from what causes all the fatal cases sank; but, as I have not found it expedient to give in detail every case operated upon in the table of 238 cases, embodying all the instances of the operation which have come under my notice, as performed by British and Jersey surgeons, I shall now offer a tabular form of the causes of death in fifty-five unsuccessful cases, giving these causes in a numerical order.

Causes of Death.	No. of Instances.
Exhaustion, &c.	14 Cases.
Pyæmia	10 "
Phthisis	5 "
Irritation, hectic, and secondary hæmorrhage . .	5 "
Shock	5 "
Causes uncertain	4 "
Chloroform and amylene combined with shock . .	3 "
Pleurisy	1 Case.
Pleuro-pneumonia	1 "
Suppression of urine	1 "
Dysentery	1 "
Diarrhœa	1 "
Anæmia	1 "
Erysipelas	1 "
Peritonitis	1 "
Tetanus	1 "
18 causes. Total, 55 ^a fatal cases.	

^a In this number are included 5 cases which died after amputation had been performed.

“Of these 238 cases amputation of the limb was performed at various periods after excision of the knee-joint on thirty occasions, on account of a variety of circumstances. Of these thirty amputations, which were all through the thigh, twenty-five recovered, and five died.

“The circumstances which necessitated amputation have already been mentioned when treating of the various cases which deserved independent remarks; but they may briefly be divided into main classes—viz., those in which amputation was performed during the time of treatment, *i.e.*, during the first four to six months after excision, and those in which amputation was not deemed advisable or imperative till a much later period. Thus, for instance, Mr. Benfield amputated almost immediately after excision in his solitary case; Mr. Birkett on the thirty-eighth day; and myself not till two years and three months after excision of the joint.

“The definite per-centage of deaths in these various cases may be obtained as follows:—

“In the whole number of cases—291—the operation of excision terminated fatally in seventy eight instances, showing a proportion of one death in 3·7 cases.

“In the more limited number of cases—238—performed by British surgeons, of which I have the fullest record, there occurred fifty-five deaths, showing a proportion of one death in 4·3 cases.

“In these calculations I have included the five cases which sank after amputation had been performed when excision had failed.

“The proportion of deaths after amputation in the thirty operations is, one death in six cases.”

Mr. Price analyzes the conditions of disease, which may be submitted to excision, in a masterly manner, and then proceeds to “discuss the circumstances which by some are said to preclude the performance of excision.” The age of the patient is here more especially referred to, and in spite of Mr. Price’s evident leaning in favour of excision, we are irresistibly led to conclude, even from his own remarks, that excision in the young, even where the line of epiphysary junction is spared, gives a result that no surgeon need care to boast of. Mr. Pemberton and Mr. Syme have set this question at rest as to the limb ceasing to develop in length. Mr. Keith’s case, much quoted to prove the contrary, fails to do so, now that the boy has grown up. At fifteen years of age his limb was seven inches shorter than the other, and is described as a mere appendage. All that can be said is, that amputation may leave matters even worse in some cases. Either alternative is devoutly to be avoided if possible by any skill and patience; and, undoubtedly, if the surgeon have the opportunity to exercise them he will

seldom be called upon to decide upon the question of amputation or excision in the child. Their powers of recovery in favourable circumstances are perfectly marvellous.

We must now take leave of a book, which must be for all time one of the authorities on its subject—imperfect, and in some respects elementary, as it is, and must, unfortunately for us, continue. Many thanks are due to Mr. Henry Smith for his share in its production. His notes are most valuable, and often supply a gap in the original. The illustrations are perfect, and make us regret that circumstances prevented the publishing of all that are referred to in the text. Opposite the title page lies the shadow of the accomplished author looking upon us with a calm reflecting eye, and already foreshadowing, in the pale cheek and wasted hand, the early doom which has been to surgery so great a loss.

A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits. Illustrated by Numerous Cases and Engravings. By WILLIAM ROBERTS, M.D., F.R.C.P., Lond.; Phys. to the Manchester Royal Infirmary; Lecturer on Medicine in the Manchester School of Medicine. Pp. 517, 8vo. London: Walton and Maberly. 1865.

WE have had much pleasure in presenting to our readers a notice of this book, which is the *bonâ fide* "work" of an English provincial physician of standing. Dr. Roberts divides his book into three parts. The first consists of four chapters, and deals with the physical and chemical properties of the urine in health and disease, as well as with urinary deposits. Chapter I. contains a summary of the properties and composition, the physiological and pathological variations of the urine; various methods of examining it; extraneous matters to be found in it; and the changes which occur in it by keeping. Chapter II. treats of the physical properties of the urine—its odour, colour, specific gravity, quantity, and reaction. It is, however, opened with the somewhat remarkable statement, that the natural odour of healthy urine "may be described as *urinous*." Decidedly so, we should say. On p. 20, Dr. Roberts observes that—"when the urine is unusually *scanty* it should be ascertained, before pronouncing it a morbid phenomenon, whether the patient has abstained from liquids above his habit,

whether water has been eliminated in excess by some other channel, as the skin or bowels." He then proceeds to remark that the urine is always scanty in cirrhosis of the liver, throughout the entire course of some forms of Bright's disease, and in the last stage of all forms of that affection; also in any condition of the heart which directly or indirectly causes passive congestion of the renal veins; in the collapse period of cholera, in the latter stages of all organic renal disease, and when any mechanical obstacle obstructs the flow of urine. Chapter III. treats of the chemical constituents of the urine, of their variations, and of inorganic deposits. In it Dr. Roberts not only gives clear and concise descriptions, but he also furnishes the reader with well executed engravings in illustration of this part of his subject. Chapter IV. treats of abnormal substances and organic deposits in the urine. Under this head we may select for a few remarks Section VI.—"Blood in Urine—Hematuria." After mentioning the mode in which an admixture of blood with the urine may be recognized by its colour, and debating the sources from which the blood may come, he very properly, in our opinion, affirms that—"the microscope is the surest means of discovering it;" and forthwith gives an enlarged engraving of blood corpuscles in the urine as they appear by the aid of that instrument. As a specimen of the concise manner in which he sets down the great variety of causes from which hemorrhage may arise we give the following classification from page 99:—

"1. *Local lesions.*—External injury, violent exercise, calculous concretions, ulcers, abscesses, cancer, tubercle, parasites, active or passive congestion, Bright's disease.

"2. *Symptomatic*—in purpura, scurvy, eruptive and continued fevers, intermittent fever, cholera, &c.; mental emotion.

"3. *Supplementary or Vicarious*—to menstruation, hemorrhoids, asthma."

In his directions for testing for albumen with nitric acid, on p. 118, he gives this very practical suggestion, the value of which we ourselves have fully appreciated:—Fill a test tube to the depth of about an inch, then, inclining the tube, pour in strong nitric acid in such a manner that it may trickle down along the sides of the tube to the bottom, and form a stratum some quarter of an inch thick below the urine. The advantage of this plan is that thus there is scarcely any mingling of the two fluids, while, if albumen be present, *three* strata or layers will be observed—"one, perfectly

colourless, of nitric acid at the bottom; immediately above this an opalescent zone of coagulated albumen, and a-top the unaltered urine. If there be only a trace of albumen, two or three minutes elapse before the opalescent zone becomes visible." Further on, at page 129, when treating of testing for sugar, Dr. Roberts remarks, with reference to Moore's test (boiling with liquor potassæ), that while it is a very convenient one, for many reasons, yet that it is wanting in delicacy, and is liable to a notable fallacy. It does not answer until the proportion of sugar rises to about 0·3 per cent, or one grain and a-half to the ounce.

Part II. discusses those urinary diseases of which the chief characteristic is an alteration of the urine. It comprises two chapters on Diabetes (I. Insuper; II. Mellitus); Chapter III. on Gravel and Calculus; and Chapter IV. on Chylous Urine. This portion of the work is very practical; is replete with records of cases; with the results of Dr. Roberts' experience, and of the experience of others. We are, however, surprised to find that the entire series of observations on diabetes and other urinary conditions, long since published in this Journal by that indefatigable follower of medical science, the Rev. Professor Haughton, are not, so far as we can see, once named or referred to in any way, although Dr. Roberts professes to give, and, indeed, with this notable exception, does give a complete treatise on the subject, and a long list of references to other writers. It cannot be that Dr. Roberts is ignorant of the papers of which mention is now made; the want of allusions to them is a considerable literary defect in his book, which we would point out with the hope that it may be remedied in his next edition. At the same time, we must note another inaccuracy; the name of our distinguished fellow citizen, Dr. Robert M'Donnell, is given as "M'Connell," and his writings are quoted under that designation no less than four times on page 183. It is remarkable that these errors occur with regard to Irishmen. Also, on page 29, the lectures of Graves are quoted with seeming accuracy, but without referring to the particular edition referred to by Dr. Roberts.

Notwithstanding these errors, however, we cannot shut our eyes to the excellencies of this work, one special feature of which is that at the beginning of each chapter he gives the bibliography of the subject. Space will not allow us to give in full, as we should greatly like to do, the formula for making bran-cakes for patients affected with diabetes mellitus. It was supplied to Dr. Roberts by

Dr. Camplin, and will be found at length in a long foot note on page 190.

Part III.—the best part of the book, in our opinion—treats of organic diseases of the kidneys. Chapter I. deals with Congestion; Chapter II. with Bright's Disease, chiefly as to its General Etiology; Chapter III. is on Acute Bright's Disease; Chapter IV., on Chronic Bright's Disease; Chapter V., on Renal Suppuration and Embolism; Chapter VI., on Pyelitis and Pyonephrosis; Chapter VII., on Renal Concretions; Chapter VIII., on Hydronephrosis; Chapter IX., on Cysts and Cystic Degeneration; Chapter X., on Cancer; Chapter XI., on Benign Growths; Chapter XII., on Tubercle; Chapter XIII., on Entozoa; and Chapter XIV., concludes with an account of anomalies in the position, form, and number of the kidneys.

We naturally turn to the account of Bright's Disease, under which title we find classed cases characterized by albuminuria and dropsy, depending on structural changes in the kidneys. Here we see that Dr. Roberts treats the entire subject from a clinical rather than from an anatomical point of view. He classes all varieties of Bright's disease under the two general heads "acute and chronic;" meaning by the former "a compact and universally recognized group, which formerly went under the designation of 'inflammatory dropsy.'" It corresponds to the acute desquamative nephritis of Johnson, to the first stage of Frerichs, and to the acute tubular disease of Dickinson. Three types of the chronic affection are recognized:—1. Cases which have lapsed from the acute state (kidney—smooth, white, generally large, exceptionally dwindled). 2. Cases which have been chronic from the beginning (kidney—granular, red, contracting). 3. Cases associated with waxy or lardaceous (so-called amyloid) degeneration of the kidneys.

Remarking that the abuse of spirituous liquors ranks probably higher than any other single circumstance as a determining cause of Bright's disease, while the use of mercury has *not* been found by wide observers to produce this effect; Dr. Roberts enters fully into the symptoms, varieties, and complications to be invariably or frequently met with, and illustrates his positions by records of well-selected cases. He specially notes the frequency of its complication with phthisis, having witnessed so many as eight cases within the last two years; declares the prognosis to be "exceedingly gloomy;" and is of opinion that in confirmed cases the treatment should aim at these objects:—"(*a*) To hinder the further extension

of the structural changes in the kidneys; (b) to prevent the occurrence of uremic and inflammatory accidents; and (c) to palliate or remove certain threatening or burdensome symptoms—*anemia, dropsy, dyspeptic and uremic phenomena, &c.*” (p. 366).

The effects of embolism in the kidney (Dr. Roberts states) are of very slight clinical importance; they pass by, in the immense majority of cases, without recognition; their occurrence is always secondary to some much more grave primary disorder, which altogether dominates the prognosis and treatment. Sometimes the larger ones go on to gangrene, and, still more rarely, to suppuration.” (p. 381).

Passing by a very interesting portion of this book—that on *Pyelitis and Pyonephrosis* (Chap. VI.)—we come to Chapter VII. on *Concretions in the Kidneys*. Here Dr. Roberts is very practical. Under the head of *treatment* we give the following specimen:—

“During the paroxysms of renal colic, the remedies indicated are:—warm baths, emollient enemata, cupping the loins, and, in highly sthenic cases, venesection. The dolorous spasm of the ureter must be combated by free administration of opium. This drug is freely tolerated in cases of this class, and full doses should be repeated until the system is plainly brought under its influence. When the irritability of the stomach is such as to prevent the absorption of the drug, it should be introduced per rectum or by subcutaneous injection. Belladonna may be substituted where opium disagrees. The secretion of the urine should be encouraged by warm demulcent drinks; hot poultices should be applied to the loins or abdomen, as the local symptoms indicate.”

He further observes that change in the position of the patient sometimes suffices to dislodge a calculus which lies upon, but has not become fully engaged in, the orifice of the ureter, while manipulation of the abdomen in the course of the ureters also facilitates the descent of the concretion. “Dr. Simpson witnessed relief follow complete inversion of the body.”

Chapter VIII. discusses the morbid anatomy and etiology of *hydronephrosis*; Chapter IX. treats of cysts found in the kidneys under four practically different circumstances:—1. Scattered cysts in kidneys otherwise healthy; 2, disseminated cysts in the atrophic form of Bright’s disease; 3, congenital cystic degeneration; 4, general cystic degeneration in adults. Chapter X., treating of cancer, states that the species of it almost invariably found in the kidney is the *encephaloid* (*fungus hæmatodes*). Several cases in

point are detailed; and one of them, of which an engraving is given, is specially worthy of note. It occurred in the Middlesex Hospital, in a girl aged six; and the tumour covered the entire front of the body from the mammary region down to and resting on the knees when in the sitting posture. In this case no cancerous deposit was found in any of the other viscera.

Passing over the discussion of "Benign Growths," in Chapter XI., we come to "Tubercle of the Kidney," in Chapter XII. This is treated of under the heads of *primary* and *secondary*; the former is the real local disease, the secondary being only a part manifestation of tuberculosis co-existing in other parts of the body. The chapter on "Entozoa in the Kidneys" (Chap. XIII.) is particularly deserving of perusal—enriched, as it is, with curious cases and numerous engravings. The parasitic worms which infest this locality are—*ecchinococcus hominis*, or *hydatid*, *bilharzia hematobia*, *pentastoma denticulatum*, and *strongylus gigas*. The two last are extremely rare; the second occurs in Egypt, at the Cape of Good Hope, and certain other hot countries, while the first is by far the most common in these latitudes. Chapter XIV. concludes this work by giving an account of anomalies as regards position, form, and number. Thus we have fixed malposition of the kidney, movable kidneys, the horse-shoe kidney, and the solitary kidney, each in turn dilated on.

We have now endeavoured to give a short outline of the character and contents of Dr. Roberts' book, which we gladly hail as a valuable addition to medical literature. As a comprehensive treatise on the subject with which it deals we cannot but look on it as the best book yet published in our language; its copious references cannot fail to give it a permanent rank in the libraries of the medical scholar, while its eminently practical, and, therefore, highly useful character, will certainly enhance its value in the eyes of the busy practitioner.

On Some of the Causes and Effects of Valvular Disease of the Heart; being the Croonian Lectures of the Royal College of Physicians for 1865. By THOMAS B. PEACOCK, M.D., F.R.C.P.; Physician to St. Thomas' Hospital, &c. Pp. 114, 8vo. London: Churchill, 1865.

ANYTHING from the pen of Dr. Peacock is likely to be worth reading. With this impression we sat down to peruse his three lectures; and we confess we have not been disappointed. Whether we regard the style, the division of the subject, or the subject-matter, the book before us is worthy of perusal. The cases given are numerous, and well selected; showing that Dr. Peacock has had that amount of practical experience which fairly warrants him in setting his conclusions before the profession. Illustrations by means of engravings are here and there met with; and we feel bound to say that they render the meaning of the lecturer much clearer than it otherwise would be.

The first lecture is on "Diseases originating in Malformation;" and on page 2 Dr. Peacock thus tabulates the causes of valvular disease or defect:—

"1. Malformation of valves, arterial and auriculo-ventricular,	} Giving rise to	{ Regurgitation, Obstruction, or Obstruction and Regurgitation.
2. Injuries of valves, arterial and auriculo-ventricular; immediate and gradual,	"	{ Regurgitation, with or without Obstruction.
3. Alterations in capacity of orifices and cavities,	"	{ Regurgitation from erosion or mal-adjustment.
4. Inflammatory affections, chiefly rheumatic, acute, and chronic.	"	{ Obstruction, Regurgitation, or Obstruction and Regurgitation."

Of these different causes of disease Dr. Peacock treats only the first three; observing that "the fourth has been so fully illustrated as neither to require nor admit of further elucidation" on his part.

The malformation, or irregular development, may appear (according to Dr. Peacock) where the valves are in excess; or where they are defective. In the former case, when there are only two semi-lunar valves, at either of the two arterial orifices, and one of

these curtains is considerably larger than the other, the larger curtain not being properly supported becomes stretched, falls below the level of the other segments, incompletely closes the orifice during the diastole of the ventricle, and so permits regurgitation. In the latter case, where the whole of the curtains are blended together and a kind of septum, perforated by a larger or smaller opening, is stretched across the aperture, more or less obstruction is necessarily occasioned. Eleven cases in point are now given. Some are cases of disease of the arterial valves; while others are cases of disease of the auriculo-ventricular valves; these cases extend over several pages, and the first lecture is concluded with remarks on other defects in the conformation of the heart or arteries which may occasion disease, such as atrophy of the valves, and want of just proportion between the size of the ventricles and the capacity of the orifices and vessels leading from them.

Lecture II. is on diseases originating in injuries, and in alterations in the capacity of the orifices and cavities of the heart. Here Dr. Peacock remarks that the valves may be indirectly injured in external accidents; but that they more generally give way under violent muscular exertion; also that these injuries may be immediate or gradual. Notes of five cases are added in support of these positions; and in summing up his remarks Dr. Peacock says:—"It has long been observed how frequently disease of the heart, and especially of the aortic valves, occurs in persons who follow laborious occupations, as smiths, strikers, and riveters, bricklayers' labourers, hodmen, &c. I have seen several cases of incompetency of the aortic valves in the 'along-shore men,' as they are called—the men who are employed to load and unload the small coasting vessels at the wharves on the banks of the Thames, the coal-whippers, and the Newgate Market and Deal porters—men who carry very heavy weights upon their shoulders, and often up slanting planks or landings, in which positions the stress upon the aortic valves must be excessive. When in Cornwall, two years ago, I met with a case of incompetency of the aortic valves in a man employed in a somewhat similar way. He was a 'coacher,' or one of the men who push the trucks containing the rock or ore along the tramways at the bottom of the mines—an operation performed in a stooping position, and which requires great force. Many of these occupations are commenced at early periods of life, and must be especially injurious to persons who have not obtained the full vigour. I have frequently known cardiac symptoms occur

under these circumstances in young persons employed in laborious occupations, and not unfrequently in girls engaged as servants or nurse-maids, before they had attained their full strength. In some of the cases there is no reason to suppose that the patients have ever had any inflammatory symptoms, and the cardiac affections are apparently due to overstraining of the valves or to some injury of the muscular structure of the heart."

Dr. Peacock proceeds with a most elaborate discussion on some of the causes of valvular defect; gives a lengthened summary which is too long to insert here; even an analysis of that summary extends over several pages, and is accompanied by a table of 63 cases of valvular disease and defect. We may merely remark that of 26 cases of aortic valvular disease, 9 or 34·6 per cent. are referred to malformation; 2 or 7·4 to injury; 6 or 23·07 to rheumatism and other combined causes; and 9 or 34·6 to various other combined causes.

Of 17 cases of uncombined mitral disease 11 or 66·7 per cent. were traced to rheumatism; and 6 or 25·2 per cent. to other causes; while of 16 cases of combined aortic and mitral valvular disease 2 or 12·5 per cent. were connected with malformation; 10 or 62·5 were referred to rheumatism and other causes; and 4 or 25 per cent. were also referred to other causes. "Of the whole 33 cases, 21 may be regarded as originating in rheumatism, or 63·6 per cent."

Lecture III. discusses some of the *effects* of diseases of the heart. Tables are given of the weight and size of the healthy heart in the two sexes, at different ages, and in persons dying from acute and chronic diseases; of the weight of the heart in different forms of disease, and when variously diseased; and of the dimensions of the heart in different valvular and other forms of disease. Dr. Peacock then makes inferences as to the weight and dimensions of that organ in different diseases; and enlarges on the symptoms and results of different forms of valvular disease. Finally, he winds up by announcing his general principles of treatment. His observations on the use and abuse of digitalis are particularly valuable, combating, as they do, some cherished opinions regarding the omnipotence of that medicine in some forms of cardiac disease. Regarding it, and other depressing remedies, he quotes the words of Dr. Stokes, "that the practitioner should never forget that local diseases, themselves incurable, may co-exist with an excellent state of the general health, for a period indefinitely long; and the

conclusion is but too obvious, that, as the disease cannot be cured, the system at large should not be tampered with." Dr. Peacock concludes with the cheerful opinion, that, we may "in many cases enable the patient to enjoy a large amount of health and vigour for many years, and in some instances probably for the full period of human life."

We look on this book as a very valuable addition to medicine and cardiac pathology; and we have much pleasure in commending it to the profession.

A Theoretical Inquiry into the Physical Cause of Epidemic Diseases, accompanied with Tables. By ALEXANDER HAMILTON HOWE, M.D., Honourable East India Company's Service. Pp. 171. London: John Churchill.

THE books upon plagues and epidemics threaten to become a plague of themselves, as the works upon these subjects, published during the past twelve months, are legion.

It will be seen, from the title of Dr. Howe's book, that he admits but *one* cause for all epidemics no matter what disease may assume the epidemic form.

Although Dr. Howe makes an apology for the irregularity in his chronological arrangement of recorded epidemics, we can scarcely accept his apology as in any way excusing him for sending out such an ill-arranged table as that which composes more than half of his book.

Dr. Howe has evidently spent a great amount of time and trouble in collecting his information, and must have compiled his notes with considerable care, as we think he has scarcely omitted any epidemic of consequence from his list. We regret we cannot regard his list of recorded epidemics at all in the light of a chronology of those events, inasmuch as it is arranged without any regard to the order of time in which they occurred; he also, by the careless huddling together of his notes, deprives us of the means of rapidly and carefully testing the truth of his statement, that epidemics occur at intervals of about eighteen years.

The main point, however, in Dr. Howe's work is, that all epidemics are to be ascribed to lunar influence. The lunar influence which he considers of chief importance, is the revolution

of the lunar node, combined with a double revolution of the lunar apse-line. He deduces from his list of epidemics, that these visitations occur at intervals of eighteen years, the variations being from seventeen to twenty years. The author states that the revolution of the lunar node is the only natural phenomenon corresponding with this period of eighteen years. Has Dr. Howe ever considered the effect of the variations in the magnetic condition of the earth, which perhaps might answer as well, granting the same margin as Dr. Howe demands for his eighteen-years period? Dr. Howe considers that the variations in the distance of the moon from the earth (24,000 miles) during its nodal revolution, must have considerable influence on the state of the earth's atmosphere, although such has never been demonstrated by science. There is no doubt, such may be the case, but on the other hand, we have the monthly variations in all states affected by lunar influence, and yet it is now well known that these variations have no influence on the prevalence of disease, although at one time such was supposed to be the case. We think that if the nodal changes produce such powerful effects, the monthly variation should have some *slight* influence.

To prove that epidemics do *really* follow definite periods in their visits, it should be shown that they follow this rule in each and every locality at every visit of each disease. We think, Dr. Howe has utterly failed to show a sufficient number of examples to prove his theory. One epidemic in China now, and another in America eighteen years hence, prove nothing as to periodicity of cause, although such may happen in a dozen scattered localities over the earth, through a succession of periods of eighteen years. Dr. Howe cites cholera as an instance of a disease following, in its progress, the direction of the regression of the lunar node along the ecliptic; to show that the direction of the regression of the lunar node guides the direction of the spread of cholera it is necessary that it should follow the same rate as the regression of the node, which it does not.

In conclusion, we regret that Dr. Howe did not carry his diligence a little further, and arrange his list so as to make it a chronological table of epidemics; if he had done so, his book would have been of considerable value for reference, independent of the wild theories it contains, which the author states he was in such a hurry to get before the public that he had no time to arrange the only valuable part of the work.

A Manual of Practical Hygiene, Prepared Especially for Use in the Medical Service of the Army. By EDWARD C. PARKES, M.D., F.R.S.; Professor of Military Hygiene in the Army Medical School; Member of the Medical Council of General Education; Examiner in Medicine in the University of London; Emeritus Professor of Clinical Medicine in University College, London. London: John Churchill and Sons, 1864; pp. 612.

It will be in the recollection of many of our readers that in 1857 a Royal Commission was appointed to inquire into the sanitary condition of the army in England. As a result of the labours of this Commission, a new edition of the *Queen's Hospital Regulations* was published by authority in 1859. The position of the army medical officer was entirely altered by the new regulations. Up to this time, although frequently consulted as to the preservation of health amongst troops and the prevention of disease, the army surgeon was, in strictness, merely officially entrusted with the care of the sick. The regulations of 1859 gave him a new position. He is ordered "to advise commanding officers in all matters concerning the health of troops, whether as regards garrisons, stations, camps, and barracks—or diet, clothing, drill, duties, or exercises."—p. 7.

Another recommendation of this Commission was that a medical school should be established, in which "the specialities of military medicine, surgery, hygiene, and sanitary science" should be taught to the young medical officers about entering the army.

Dr. Parkes' work, now before us, has for its object to carry out the wishes of the Commissioners as regards sanitary science, by providing a text-book of hygiene, illustrated by examples drawn from army life, for the gentlemen attending the army medical school.

It is true, that in writing this work the author has had to deal with but one sex, a certain age, and a particular calling; he conceives, however, that he is entitled to designate his work as one on general hygiene, as the principles of hygiene are tolerably fully discussed. We conceive that he is indeed justified in assuming the general title, and we assure our readers that they will find in the pages of Dr. Parkes' book not only much that is instructive and entertaining, but a great deal of information quite as important to the civil as the military medical man. The work is divided into

two books. In the first, the chief subjects of hygiene—water, air, food, sewerage, clothing, climate, &c.—are arranged in a convenient order, and discussed in a very clear and masterly style. Some other subjects, such as meteorology, statistics, physical training, are introduced, which it is of consequence that medical officers should be familiar with.

These chapters contain much that is useful, not only to the army surgeon, but to all professional persons who have not books on these topics at hand. The book is indeed valuable as a work of reference, and contains a great deal compressed into a small space.

The second book is more special in its character, and consequently less important in the eyes of the civilian medical man; it is devoted to the service of the soldier. The first chapter is given to the recruit—his weight, height, age, training, &c.; the remaining chapters to home and foreign service, service on ship-board, and service in war.

When speaking of foreign service the author mentions many important particulars touching each station garrisoned with British troops. Thus of Malta he says:—

“MALTA.

“Size 17 miles by 8. Usual peace garrison=6000 to 7000; period of service, three years; population (civil) in 1851=98,021.

“*Geology*.—Soft, porous rock; the greater part is sandstone, resting on limestone; in some parts marl and coral limestone over the sandstone. In the centre of the island, at Citta-Vecchia, there is, in order from the surface, alluvium, upper limestone, red sand, marl, sandstone, and lower limestone. Valetta is on thin alluvium, with thick sandstone below, and beneath this the lower limestone.

“*Climate (at Valetta)*.—Mean of year, 68°; hottest month (July), 77°; coldest (January), 57°; amplitude of the yearly fluctuation, 20°; extreme yearly range (from highest to lowest temperature in shade), 62°; from 100° in July to 39° in January; mean yearly range, about 50°; extreme monthly range (*i.e.*, from highest to lowest in month), about 25° to 35°.

“Undulations of temperature are frequent, and there are often cold winds in winter from N.W. The south-east wind is an oppressive sirocco, raising the temperature to 94° or 95°. It is chiefly in the autumn, and blows for from 70 to 80 days every year. At Citta-Vecchia (600 feet above the sea) the temperature is lower and the air keener. Rain-fall about 32 inches. Chief rain in November, December, and January; less in February and March; small in amount in the other months. From June to August almost rainless.

“*Humidity*.—(Mean of 1859-60); observations at 9.30 A.M. and 3.30 P.M.

	Dew-point.	Grains of Vapour in a cubic foot.	Relative Humidity.
Mean of year,	60·5	5·86	62
Highest in year (August),	72·7	8·73	...
Lowest in year (February),	49	3·96	...

“Malta thus appears to be a dry climate, *i.e.*, with little relative humidity.

“*Sanitary Condition.*

“Much has been done of late years, and, as far as external cleanliness goes, Valetta is very clean. Water supply from rain and springs (the largest of which is in the centre of the island, and the waters of which are led by aqueduct), is not very deficient in quantity (8 to 10 gallons per head), and, except in some places, good in quality, though the rain-water contains chlorides from the spray falling on the roofs of buildings. Some of the tanks are too near the sea, which percolates into them. The tanks require, however careful looking after. Within the lines there are 272 public and military tanks, with storage for 55 millions of gallons, and 4,294 private tanks, with storage for 323 millions of gallons. The military tanks, if full, would give 6 gallons of water per man daily for eleven months, but even now the water often falls short. The water is now carried everywhere by hand, and the drinking-water for the men is not filtered. The sewers in Valetta are bad in construction and outlet, and much typhoid has been and is still caused in consequence. In many cases ‘they are nothing but long cesspools.’^a

“The barracks are bad, many casemates being used, and buildings intended for stores, and not for habitations. In some cases all sanitary considerations have been sacrificed for the purposes of defence. They are built of soft sandstone, which both crumbles and absorbs wet. The ventilation of the casemates is very bad. The Barrack Commissioners, in their Report, recommended that in every way which can be done the ventilation should be improved by admitting the wind, especially from the north, and that each barrack will require a separate plan to meet the particular case. They recommend that air-shafts shall be made much larger than ordered for home service, *viz.*, 1 square inch for every 20 cubic feet of space, or for a barrack of twelve men with regulation space ($7200 \div 20 =$) 360 square inches ($= 2\frac{1}{2}$ square feet) of outlet opening. At the present time the amount of cubic space is below the home service amount (600 cubic feet) and the superficial area is very small, one-fourth of the men having less than 40 square feet each.

^a Barrack Commissioners’ Report, p. 111.

"Means for ablution are very deficient. Urinals and water latrines are made of porous stone, and are also bad in construction.

"It is therefore evident that the condition of Malta is a parallel to that of Gibraltar, and very much the same diseases may be expected, viz., typhoid fever from bad drainage, and lung disease from the faulty ventilation. As the water is less impure, the amount of dysentery may be expected to be less.

"In time of war, the dangers at Malta would be the same as at Gibraltar; the aqueducts might be cut by a besieging force, and the water supply restricted to the tanks. Although these are supposed to hold a large quantity, they are not kept full, and could not, perhaps, be rapidly filled. The garrison might be driven to distil the sea water. The Barrack Commissioners very properly strongly advise that a tank inspector should be appointed. A still more serious danger would be the overcrowding of a war garrison. Doubtless, in case of a war, the garrison would only be concentrated in the lines when the siege commenced, but the crowding during a siege of three or six months might be very disastrous. This danger should be provided for beforehand by a clear recognition of what accommodation would be wanted for war, and how it is to be obtained without violating either the conditions of health or of defence."

We are happy to learn from Dr. Parkes that the Director-General has recommended, and Lord de Grey sanctioned, the issue of a box containing sufficient apparatus and reagents to enable medical officers to perform chemical processes required in the analyses of water and air, and in the examination of food.

We can indeed say that we have derived much pleasure and instruction from perusing Dr. Parkes' book, and we look upon it as a work not only likely to improve materially the position of the British soldier, but as one which may be consulted with profit by any person interested in the great questions of sanitary science and the prevention of disease.

Tetanus.—*Eine Physiologische Studie.* Von DR. JOHANNES RANKE, Privatdocent der Physiologie und Assistent am Physiologischen Institute in München. Leipzig: Verlag von Wilhelm Engelmann, 1865, pp. 468.

Tetanus.—*A Physiological Study.* By DR. JOHANNES RANKE, Lecturer on Physiology and Assistant in the Physiological Institute in Munich. Leipzig: Engelmann, 1865, pp. 465.

THE title of the book before us requires explanation: it is a physiological inquiry into the circumstances attendant on muscular contraction. It is, in fact, a work on the physiology and physiological chemistry of muscle and muscular action.

It is a work of great merit, and one which places its author in the foremost rank among the physiologists of Europe; it gives evidence at once of that careful research which characterizes German investigation, and of genuine originality.

In a review like the present we can only lay before our readers an epitome of the contents; but we strongly recommend those interested in the important subject treated of in Dr. Ranke's book to peruse it for themselves.

Dr. Ranke commences by giving a brief history of the chemistry of muscular action up to the time when he commenced his researches. In this introduction he reminds us that one of the chief problems for physiological investigation has always appeared to be the solution of the question as to the cause by which muscular action is brought about.

There was a time when the consideration of this and similar problems was entered upon with *à priori* conjectures—with philosophic systematic speculations. Many groundless hypotheses thus took their origin.

F. Magendie, at the time when he strove to introduce an exact method of physical investigation into physiology, spoke of the condition of this question briefly and with candour as follows:—"What changes take place in the constitution of the muscles during the state of contraction? We do not know; and in this respect muscular contraction does not differ from the vital processes, of which no explanation whatever can be given."^a

-Before the researches of Liebig concerning flesh meat no

^a Grundriss der Physiologie Translated by Heusinger, 1820.

chemistry of muscle existed; how could there be then a chemistry of muscular tetanus? The first commencement of any such is found in the researches of Liebig just mentioned.*

The following is a sketch of the various amount of kreatine in different kinds of flesh:—

The order in which Liebig arranges the flesh of mammalia according to the amount of kreatine in each, viz., so that that flesh which is richest in kreatine takes the first place, and that containing the least the last, appears to be the same in which they must be arranged with reference to the amount of motion which these animals are in a position to perform during life in relation to the mass of their muscles.

The order is as follows:—The martin, horse, fox, roebuck, stag, hare, ox, sheep, pig, calf.

At page 36 of the work already quoted Liebig says:—

“The difference in the amount of kreatine is striking, even in the same class of animals. The flesh of a fox which had been fed for two hundred days upon flesh meat afforded not a tenth part as much kreatine as was obtained from an equal weight of the flesh of foxes killed in hunting.

“The quantity of kreatine in the muscles of an animal is in an obvious relation to the amount of fat or to the causes which determine the putting up of fat. From fat flesh mere traces only are often obtained, but always far less kreatine comes from lean, the quantity of fibre being equal. The fed fox already mentioned yielded over one pound of abdominal fat, whilst the fat of coursed or hunted foxes was scarcely appreciable by the eye.”

In the heart of the ox kreatine is present in considerable quantity, although otherwise ox-flesh takes a low place in the range of those containing kreatine.

The flesh of animals which are in a condition to perform more muscular work in proportion to the bulk of their muscles, contain more kreatine.

By means of repose—the cause of an accumulation of fat—the quantity of kreatine in the flesh diminishes. By means of coursing and hunting—muscular work—it increases.

The most actively-employed muscle of the organism—the heart—

* *Chemische untersuchung über das Fleisch und seine Zubereitung zum Nahrungsmittel.* Heidelberg, 1847, C. F. Winter. *Ann. Ch. Pharm.*, lxii., 257.

contains a greater quantity of kreatine than the less actively-exerted muscles of the trunk.

It is clear that these observations must be thus explained:—

Muscular contraction is connected with an increase of the kreatine in muscles.

There was proof that during tetanus appreciable chemical changes went on in muscle. Thereupon a foundation was gained for assuming that one would be in a position through further researches in this direction, to find a physical groundwork for the production of muscle force in the forces set free in its disintegration and oxydation.

The kreatine could only be looked upon as the product of the disintegration of the muscular substance.

The path of direct research concerning muscular action, which Liebig had opened with this first result of his enquiry, was not the only one which was mentioned by him with the object of establishing a chemistry of muscular tetanus. He had already, in his *Thier-Chemie*, page 202 ff, bestowed upon physiology two other methods, viz., the method of the comparative determination of urea and the method of determining the carbonic acid of respiration, with the same object.

When Liebig came to the conclusion that all the nitrogen resulting from the nitrogenous material disintegrated in the body makes its appearance in the urine, he thereby gave the means of inquiring how far the work done by muscle is connected with the consumption of muscular tissue, which must yield nitrogenous products as the result of disintegration. Indeed, this very method of investigation must appear all the more inviting as one must admit that changes, which may easily escape detection in a tissue so difficult to investigate as that of muscle, may be made out without much difficulty by the investigation of the urine after the comparatively easy chemical method of Liebig.

Besides that Liebig, in his theoretical observations on his experiments on animals, had laid much stress on the connexion between the exhalation of carbonic acid and the interchange of matter, he had also pointed out the method of investigation of the amount of carbonic acid separated during respiration as a means of determining the influence of movement in general on the interchange of matter of the organism; and thereby, secondarily, of that in the organs of movement, viz., muscle.

By creating this method of inquiry he opened up the question as

to the influence of muscular movement on the chemical constitution of muscle, and along with that the whole organism in the true sense of the word. We shall see that this question was approached from different sides promptly, and indeed, in some instances, too precipitately, for the accurate determination of the replies that were demanded.

All the three above-mentioned methods of investigation which Liebig had pointed out were forthwith tried to be turned to account by different investigators of the chemistry of tetanus.

C. G. Lehmann (Wagner's *Hdbch. d. Phys.*, Band II., s. 21; and Lehmann, *Phys. Chemie* Bd. I., s. 164, 1844) first of all gave his attention to the determination of the influence which muscular motion possesses over the elimination of nitrogen by the urine, viz., in the excretion of urea.

He came to the conclusion that active bodily exercise gives rise to an increase of the urea excreted.

The diet being nearly the same, he eliminated, as a general rule, 32 grammes of urea during a day of repose; but after considerable bodily fatigue, 36-37 grammes.

The researches of J. F. Simon (*Hdbch. d. angew. Med. Chem.*, 1842, II., 368) gave also an increase of urea excretion in consequence of persevering exercise. In this case also there was not given a complete determination of the relation as to diet.

The question then seemed settled; and what had been looked on as a reasonable postulate appeared to become a stronghold of physiology.

The discovery made by E. du Bois-Reymond and G. V. Liebig (*files*) (in prosecution of the researches of Humboldt, Fontana, Tiedemann, and others) on the influence of the chemical composition of the atmosphere on muscular movement, especially the movement of the heart, strongly supported the idea of profiting by the methods of estimating the carbonic acid separated by respiration, as well as in connexion with the relative interchange of it in the muscle itself. This discovery was that the muscle of the frog, so long as it still reacts with electricity, absorbs oxygen and gives off carbonic acid, and that a muscle placed in nitrogen exhales carbonic acid until the extinction of its capability of being made to contract.^a

Matteucci also made this observation, and showed distinctly that

^a Müller's Archiv., 1850. 393; Berl. Acad. Ber., 1850, 339; Pharm. Centr., 1850, 769; Instit., 1851, 118.

an increase of the separation of the carbonic acid of muscle takes place in consequence of electrical tetanus.^a

Valentin's researches corroborated the results already known; he further found that the muscle, after the cessation of its irritability, gives off carbonic acid and takes up oxygen.^b

The researches as to the total amount of carbonic acid given off in respiration under the influence of exercise were for the most part carried on by Scharling^c and Vierordt.^d

They both found that after violent exercise the exhalation of carbonic acid seemed absolutely increased.

Prout (*Schweiggers Journ.*, XV., 60), on the contrary, found at the commencement of moderate exercise an increase of the carbonic eliminated in respiration, while on the other hand it was diminished by fatigue: even at the very first violent bodily exercise caused a diminution of it.

Séguin^e had, moreover, already shown that during strong muscular movement more oxygen is consumed than during weaker ones.

More recent experiment verified the observation of the increase of carbonic acid under the influence of muscular movement.

The path also of direct chemical research concerning muscular tissue under the influence of tetanus was discovered, and gave some important results. E. du Bois-Reymond showed, that in consequence of the action of muscle an acid reaction appeared in it, whilst muscle during repose gave a neutral reaction. According to Liebig's researches—which, however, confirmed the fact as to the neutral reaction of fresh muscle—the acid existing in the muscle juice proved to be lactic acid. The phenomenon, therefore, observed by E. du Bois-Reymond of the juice of muscle becoming acid in consequence of tetanus rests on the formation of lactic acid from other elements of muscle.

Helmholtz^f made the discovery that the fixed substances held in solution in the fluid expressed from flesh, which were extracted from muscle tetanised to exhaustion, were different from those

^a Comptes rend. xlii., 648 ff.

^b Archiv. f. Phys., Heilkunde, xiv., 431 ff., 1857.

^c Berzelius Jahresbincht, xxiii., 602; xxvi., 829. J. pr. chm., xlviii., 435. Ann. ch. pharm., xlv., 214 ff.

^d Archiv. f. Phys., Heilkunde iii., 536 ff. Physiologie des Athmens. Karlsruhe, 1845. Respiration in Hwbch. d. Phys., 1844, 828 ff.

^e Memoirs de l'Academie de Paris, 1790.

^f Müller's Archiv., 1845.

which could be obtained from the similar muscle of the same animal which had been left in a condition of repose.

This was for the time the first direct proof that the contraction of muscle was actually accompanied by a chemical change in the substance of muscle. The researches of Liebig regarding kreatine took place later. Helmholtz found that in muscle which had been tetanised the spirituous extractive matter was increased, while the matters soluble only in water were diminished.

The further discovery of Helmholtz* that muscle produced heat during contraction furnished proof that there was not only a disintegration of tissue during tetanisation, but that an increased oxydation takes place also.

Liebig's idea was, that all the nitrogen arising from the disintegration of the nitrogenous tissue of the animal organism was eliminated by the urine. It was in this way, as already mentioned, that the analysis of the urine became a means of determining quantitatively probable fluctuations in the amount of disintegration of the nitrogenous structures of the body, especially of the muscles, as might be expected to result from tetanus. We saw that the researches of Lehmann and Simon, which have been already mentioned, have been based upon this idea of Liebig's.

It is obvious, that if the urea were no measure of the change of tissue, in the sense supposed by Liebig—that if, besides, the separation of nitrogen through the kidneys, another elimination of nitrogen, not capable of being estimated, took place through some other channel—then the observations of Lehmann and Simon would prove nothing at all regarding the chemistry of muscular tetanus, even although correct as regards other considerations. The increase described by the above-mentioned writers in the elimination of nitrogen in the urine in consequence of augmented muscular movement was really considered as depending upon the increase of the interchange (wear and tear) in the work-doing nitrogenous organs, viz., the muscles.

If the proposition of Liebig were not tenable, then this specious argument would be good for nothing; muscular movement, indeed, might be able to change the relation of the actual separation of nitrogen in a gaseous form to the separation of it through the urine, somewhat to the advantage of the latter. Even although the foregoing observations were quite trustworthy, it would not on

* Müller's Archiv., 1848.

that account justify any assumption as to an increase in the interchange (wear and tear) of the nitrogenous structures of the body through the occurrence of tetanus.

The proposition that the urea was the measure of the disintegration of tissue seemed actually to totter.

Experiments upon diet, apparently conducted with accuracy, always gave a deficit in the elimination of nitrogen through the channel of the kidneys compared with the nitrogen taken in with the food.

The observations of Barral,^a made upon himself and those of Th. L. W. Bischoff,^b appeared the most exact. Both these authors found a deficiency of nitrogen. So likewise did Bidder and Schmidt.^c For a cat an equal quantity seems to have been proved; but this observation stands quite alone.

In addition to this Regnault and Reiset,^d in their researches on the respiration of animals, had recorded in many cases an exhalation of nitrogen during respiration corresponding to the deficiency of nitrogen in the urine, whilst in other cases, on the contrary, there was an absorption of gaseous nitrogen throughout the organism.

Matteucci and Valentin, in the experiments already alluded to on the carbonic acid given off from the muscles, had attempted to show that a separation of nitrogen also took place; the latter, however, conceived that in this observation an error had occurred in the experiment.

In the researches of Valentin regarding respiration, we find a heading, "Excess of Nitrogen and Error of Observation."^e The excess of nitrogen is assumed to be supplied through the respiration.

If these observations, of which we only quote the more important, should be verified, then the method of determining the nitrogen in the urine as a confirmation of the examination of the amount of disintegration of the nitrogenous tissues of the body would fall to the ground.

Meanwhile, the observations of Lehmann and Simon got

^a Ann. Ch. Phys. (3), xxv., 129; J. pr. Chem., xlviii, 257. The conclusions, also: Compt. Rend., xxvii., 361; and Pharm. Centr., 1848, 929.

^b Harnstoff als mass de stoffwechsels., 1853.

^c Die verdauungssäfte und der stoffwechsel, 1852.

^d Ann. Ch. Phys. (3), xxvi., 299; Ann. Ch. Pharm., lxxiii., 92, 129, 257. The results: J. Pharm. (3), xvi., 297; Pharm. Centr., 1849, 683.

^e Die Einflüsse der vagusläbmung, &c., 1857.

additional support from those of H. Beigel,^a and W. Hammond,^b and later from Genth^c also.

These researches were, however, all rendered valueless so long as no proof could be adduced that no nitrogen was eliminated by the process of respiration.

Furthermore, on quite different grounds, all the foregoing researches concerning change of tissue, and the conclusions based on them, are worthless.

Before the experiments of Th. L. W. Bischoff and C. Voit^d on diet, no experimenter had made really accurate researches in this direction. Everything which had hitherto been done concerning this subject was practically quite eclipsed by their extreme care.

In future similar investigations can only be carried on after their method.

They showed that the deficiency of nitrogen in the excreta so often asserted to exist did not exist in reality. In consequence of this Liebig's proposition was not so much reinstated on a basis of more exact enquiry as established as a new truth. The nitrogen in the excreta is a measure of the disintegration of the nitrogenous elements of the body.

C. Voit^e undertook anew the researches of Lehmann. They proved before his accurate experimentive criticism not only to have been carried on on a faulty principle, but to be erroneous in their results also. C. Voit adduced proof that the increase of elimination of nitrogen (that is urea) which had hitherto been maintained to occur in consequence of muscular work, did not exist.

From all that had been hitherto gained nothing remained to serve as a basis for a mechanical explanation of the work done by muscle, except the results gained from muscle itself.

It is obvious that the observations of Helmholtz, of the increase of temperature in muscle during action, cannot be reckoned under this head. They show an increase in the oxydation of muscle in action, but nothing more. The forces set free in this case are not,

^a Untersuchungen über den Harn und Harnstoffmengen, in den Verhandlungen d. k. Leopold. Akad. d. Naturf. Bd. 25 abthl. 1, 477, 1855.

^b Americ. Journ., Jany., 1856.

^c Untersuchungen über den Einfluss des Wassertrinkens auf den Stoffwechsel. Wiesbaden, 1856.

^d Die Gesetze der Ernährung de Fleischfressers, 1860.

^e Untersuchungen über den Einfluss des Kochsalzes, des Kaffees und der muskelbewegung auf den stoffwechsel, 1860.

indeed, applied to the mechanical performance of the muscle, but to its increase of temperature.

There remained only, as established facts, the following, viz.:—

1st. In consequence of tetanus, muscular substance, which during repose has a neutral reaction, becomes acid.—Du Bois-Reymond.

2nd. Muscular work seems to increase the quantity of kreatine in muscle.—Liebig.

3rd. The relative proportions of extractive matters in muscle at rest and tetanus are different. In the latter there is found an increase of the alcohol extractives and a diminution of the water extractives.—Helmholtz.

4. Muscle during tetanus gives to the atmosphere more carbonic acid than when in a condition of repose.—Valentin and others.

This was the state of the question in 1860, when Ranke took up the subject.

Since then the most important facts added are the discovery of a true sugar in muscle by Meissner, and the confirmation by Sarokow of Liebig's observation as to the increase of kreatine in muscle in consequence of work accomplished.

We are now in a position to consider what are the results brought to light since 1860, by our author, and this we hope to do at a future period.

(To be continued.)

“RECENT WORKS ON DISEASES OF THE LUNGS.”

1. *Remarks on the Antecedents and Treatment of Consumption.* By CHARLES DRYSDALE, M.D., M.R.C.P. Lond.; F.R.C.S. Eng.; Physician to the Farringdon Dispensary, London. M'Gowan & Danks, Great Windmill-street. Pp. 12.
2. *Consumption, as Engendered by Rebreathed Air and Consequent Arrest of the Unconsumed Carbonaceous Waste; its Prevention, and Possible Cure.* By HENRY MACCORMAC, M.D. Second Edition, Enlarged and Revised. London: Longman, Green, Longman, Roberts and Green. 8vo, pp. 240. 1865.
3. *The Elements of Prognosis in Consumption, with Indications for the Prevention and Treatment.* By JAMES EDWARD POLLOCK, M.D. London: Longman, Green, and Co. 8vo, pp. 432. 1865.

4. *Australia for the Consumptive Invalid; The Voyage, Climate, and Prospects for Residence.* By ISAAC BAKER BROWN, Jun. London: Hardwicke. Post 8vo, pp. 137. 1865.
5. *Winter Cough, Catarrh, Bronchitis, Emphysema, Asthma.* By HORACE DOBELL, M.D. London: Churchill and Sons. Pp. 182. 1866.

THE first essay on our list was read before the Harveian Society last year; it is not very voluminous, but it contains a good deal of matter worthy of being perused and carefully reflected upon by others besides members of the medical profession.

Dr. Drysdale believes that in order to speak of the treatment of consumption, it is absolutely essential that we be well acquainted with the antecedents of the disease. If anything in medical science is certain, it is that if this frightful plague, from which mankind suffers so much, is ever to be staid, it is by looking to its antecedents, and endeavouring to remove the causes which give rise to it, that alone we can hope to effect such an object. Hereditary transmission, intemperance, excessive tobacco-smoking, various personal antecedents, as sexual excess, disappointed love, topical influences, are spoken of by the author, but it is to his excellent remarks on poverty, as a cause of consumption, that we would especially direct attention.

“Poverty.”—One of the most frequent antecedents of phthisis pulmonalis in Europe is poverty, or the inability to obtain the necessities of existence by means of labour or otherwise. Lombard, D’Epiné, and Lebert have given sufficient evidence that this disease is much more prevalent among the poor than among the upper classes. This cause of consumption has not, in my opinion, been sufficiently dwelt upon by medical authors who have treated of the disease. The way, too, in which poverty, or the want of a sufficient supply of the essentials of nutrition, acts in producing consumption requires explanation. It would appear from Livingstone’s account that the savage tribes of South Africa are subject to periodical want of food. In such cases the natives become exceedingly emaciated, and often die in great numbers of starvation. Nevertheless, this want of food does not develope in their hardy frames tubercles of the lungs, and we must, consequently, look to other causes beyond that of starvation to account for the large amount of disease in our civilized and industrial manner of existence. With us want of sufficient food is one of the most frequent antecedents of consumption. To recall this subject vividly before the mind I shall quote a few passages. The following is taken from one of

the daily London papers of the 26th October, 1864:—‘An inquest was held at the Canrobert Tavern, Bethnal Green Road, last evening, on the death, from want, of Emma Jackson, aged forty years. Martha Burkhardt said the deceased used to bind two pair of boots a day, and she was paid $2\frac{1}{2}$ d. for one pair and 3d. for the second, which was of a better kind; she never earned 6d. a day, but she used to say she earned more, “so as to keep up the look of the thing.” Dr. G. Rolf said there was disease of the lungs and heart; the *cause of death was the lung disease*, accelerated by exposure and want of the common necessities of life.’

“In the sixth report of the medical officers of the Privy Council for 1863 there is a report by Dr. Edward Smith, ‘On the Food of the Labouring Classes,’ in which I find a table of the cost of food of various classes of our workpeople:—Silkworkers, 2s. $2\frac{1}{2}$ d. a week; needlewomen, 2s. 7d.; kid-glovers, 2s. 9d.; shoemakers, 2s. $7\frac{1}{2}$ d.; stocking-weavers, 2s. $6\frac{1}{4}$ d.: and Dr. Smith says:—1. No class under inquiry exhibited a high degree of health. 2. The least healthy are the kid-glovers, needlewomen, and Spitalfields weavers. 3. The average quantity of food was *too little for health and strength*. Of the needlewomen he says: ‘This is the lowest paid class included in my inquiries; ordinary hours of work ten to twelve hours. The average income was only 3s. $11\frac{1}{4}$ d. weekly per adult.’ Mr. Godwin, in his work entitled ‘Another Blow for Life,’ says:—‘Among the causes which lead to the evils we are deploring, we must not overlook the gradual increase of children, while in the case of the labouring man the income mostly remains the same. The want of *providence* on the part of these men is often commented on with good intentions; but, whilst admitting that better use might be made of their incomes in many instances, we must not omit to remember the cost of living in London, especially with a large family of children. As the children increase in numbers the wife is prevented from adding by her earnings to the income; and many years must elapse before the children can be put to work.’ It will be observed that Mr. Godwin does not include in his definition of *providence* any thought on the part of the poor concerning the number of children they shall bring into their own unfortunate position. In fact, this view of the question is studiously avoided by almost all writers even of the greatest eminence, except by those who, like Mill, Darwin, and a few others, adhere to the truths pointed out by the great discoverer of the law of population, Malthus.

“Great towns, then, cause consumption and deterioration of health, by reason of the unwholesome occupations in which they abound. Nor are these unwholesome occupations well remunerated. The fact of their being unwholesome keeps all persons who can afford a choice out of their ranks; but the destitute offspring of a Hampshire or Wiltshire country labourer, with a large family, has no choice. He is penniless, and without even

the rudiments of education, and is therefore glad to accept any employment, however deadly, from the grinders' trade of Sheffield to that of the slopworker of Whitechapel, and thus helps to recruit the large ranks of the victims to this fell disease.

"It may be seen that the amount of pulmonary consumption in this country is in great measure directly attributable to the low wages of the unskilled rural labourer; and the unhealthy towns merely allow a greater number of persons to live a few years in a frail state, without really improving the condition of the working classes. Consumption is one of the positive checks, and the saddest of all, to population; and has in great measure taken the place of those rapid epidemics which used suddenly, as they still do in India, to decimate a half-starved, because redundant, population. Mr. Darwin, in his 'Origin of Species,' as well as Mr. Huxley and Sir C. Lyell, have laid great stress upon the enormous power of reproduction possessed by all animals and vegetables, when placed in positions where their food can be supplied *ad libitum*. The race to which we belong is no exception to this rule, and we have at this day notable examples to illustrate its truth, in the enormous rapidity of reproduction of the inhabitants of the United States and our own Australias. We have there seen the fecundity of that branch of the race to which we belong so great as to enable the early colonists of Massachusetts, &c., to double their numbers independently of immigration, and by the mere powers of a fecundity of which the human race is but one example, in periods in many cases less than twenty-five years. Thus, in the United States before the year 1783, according to J. Garnier, in his work 'On Population,' war and different circumstances were an obstacle to immigration, and carried off from that country more than Europe furnished it with. The French Revolution was an occasion of migration to America; but this movement was soon interrupted by the war in 1793; and from this time until the Peace of 1815 Europe furnished very few emigrants to the States.

"In 1782 the population of the States was 2,389,000

1790	"	"	3,929,000
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1800	"	"	5,305,000
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or more than double in *eighteen years*. This great fecundity of the human race has long been known to be the cause of the low wages which it has been my endeavour to prove to be the main cause of consumption at this time in this country. 'Nature,' says the great discoverer of the *law of increase*, Mr. Malthus, in his "Essay on Population," 'cannot be defeated in her purposes. The necessary mortality must come in some form or other; and the extirpation of one disease will only be the signal for the birth of another, perhaps more fatal. We cannot lower the waters of misery by pressing them down in several places, which must necessarily

make them rise somewhere else. . . . I believe,' he adds, 'that it is the intention of the Creator that the earth should be replenished : but certainly with a happy population, not an unhealthy, vicious, and miserable one.' Unfortunately, the poor are but too often misled by the educated, as to their duty with regard to posterity, as is forcibly remarked by the most eminent thinker of his time, Mr. John Stuart Mill, in his 'Principles of Political Economy : ' 'While a man who is intemperate in drink is discountenanced and despised by all who profess to be moral people, it is one of the chief grounds made use of in appeals to the benevolent, that the applicant has a large family and is unable to maintain them. Little advance can be expected in morality until the producing large families is regarded with the same feelings as drunkenness or any other physical excess ; but whilst the aristocracy and clergy are foremost to set the example of incontinence, what can we expect from the poor ?'

"I have not been able to ascertain the number of deaths from consumption in France. If we measure these, however, by the rate of mortality (by Dr. Christison's method) prevailing in that country, there has been a great diminution of late years of the ravages of the disease in France. Before the Revolution, the average of life, *vie moyenne*, according to the 'Annuaire du Bureau des Longitudes,' was 29 at birth, and it is now 39. It is interesting to look at the concomitant circumstances. M. Joseph Garnier tells us, that from 1806 to 1810 there was 1 birth to 30 inhabitants in France ; from 1826 to 1830 there was 1 birth to 33 ; from 1841 to 1845 there was 1 birth to 35 ; and from 1846 to 1850 there was 1 birth to 37 inhabitants. This explains the increased comforts of the French population. The capital of the country is increasing, and the numbers remain nearly stationary, hence the average dividend is increasing.

"I have entered thus at length into the question of the dependence of wages on numbers and capital, in order to give the theory of the causation of the great mass of the cases of the consumption occurring in this and similar industrial countries. The theory is, I believe, so well made out, that we may look forward very hopefully to an immense diminution in future of the mortality from such diseases as are caused by overcrowding and unhealthy town-occupations, until, at length, society may solve the problem how to keep the producer in the highest health and at the same time to increase the productive powers of labour.

"A remarkable confirmation of the truth of the account I have given of the causation of consumption has recently been furnished (unconsciously, I am informed by the author), from the answers obtained by Dr. Edward Smith to his numerous questions put to 1,000 hospital patients affected with phthisis pulmonalis. Thus, he found the notable fact, that the average number of children which the parents of these patients

had produced was 7·5. In some of the families there were as many as twenty-three children. It is easy to conceive to what privations in early life many of these unfortunate patients must have been exposed, from the fecundity of their parents."

The truth is, that the prevention of pulmonary consumption, in so far as this disease depends on poverty, the results of overpopulation, does not lie immediately in the hands of physicians. Medical men can, at the best, only indicate, as Dr. Drysdale has done, this cause of disease: before the cause can be removed the masses must be awakened to the fact that the reckless imprudence, the criminal selfishness, which prompts them, regardless of consequences, to propagate a progeny they cannot feed, shows a want of morality as great as a life of debauchery and vice.

There lies within the domain of medicine, more strictly speaking, a cause of pulmonary phthisis, which we are surprised that Dr. Drysdale has not mentioned. The name of Dr. Drysdale is already known to the readers of this Journal as one who has spoken boldly and strongly against the abuse of mercury, we are surprised therefore, that he does not allude to this, or that he does not dwell more upon it in his pages. We would hope that he may, on another occasion, deal with this important part of the subject, for it is our firm conviction that, of all the causes of consumption which can be regarded as preventable by medical men themselves, this is the most important. We recommend Dr. Drysdale's pamphlet to our readers as one which, without being diffuse, contains much important information.

Our readers will not require from us a lengthened notice of Dr. MacCormac's work, the second on our list; it has reached a second edition, and been thought worthy of translation into two continental languages. Moreover, its talented author, having convinced himself of the immense saving of life which would follow the practical adoption of his views, has considered it his duty, not only at much trouble, but, we must add, sometimes in the face of very uncourteous criticism, to draw the attention of scientific societies to his law:—

"Wherever the air, habitually respired, has been respired in whole or in part before, there tubercular deposits are found; and wherever the air, habitually respired, has not been respired in whole or in part before, there tubercular deposits are impossible, and consumption and scrofula are unknown."

Tubercle he believes to consist of the hydrocarbonaceous waste of the tissues, deposited in the lungs or other structures instead of being burned off as it would be by an adequate supply of oxygen. Several works on the treatment of consumption have lately issued from the press. If these be compared with those which appeared some years ago we feel convinced that nothing will strike the reader more forcibly than the way in which the writers insist on the pre-eminent importance of free, fresh, open air. In the streets of every large city we constantly meet with phthisical sufferers sent out by their medical advisers, who, some years ago, would have been warned not to trust themselves beyond the equable temperature of their room; and in the work which is recognized as the exponent of advanced medical opinion we find it stated that:—"The broadest fact established regarding the exciting cause of the tuberculous deposit is, that the domesticated animal is more liable to tubercular disease than the same animal in a wild state—no doubt due to deficient ventilation and the abeyance of normal exercise of the pulmonary function."—Aitken, second edition, Vol. II., p. 412.

These and many other facts which could be brought forward prove the soundness of the practice for the enthusiastic advocacy of which we believe both the profession and the public are greatly indebted to Dr. MacCormac. We would not, however, be understood to commit ourselves to the admission, that the breathing of air which has been, in whole or in part, prerespired is the *exclusive* cause of tubercle. There are many difficulties in the way of doing so, to which we cannot here advert; but, admitting Compté's dogma,^a as quoted by Dr. MacCormac, we believe that his doctrine fails to explain the fact, that of several persons exposed as nearly as possible to the same influences, *quoad* air, one alone may become tuberculous.

We are sorry to find Dr. MacCormac, for whom we entertain the greatest respect, maintaining that the opinion formed by conscientious and painstaking physicians, after years of careful observation in thousands of cases, is as childish and irrational as the "superstition of our ancestors in respect of the king's touch" (page 64), and that "all the cod-fish that swim the mighty ocean, were they converted into oil, could not, would not, relieve or avert, were it but in a single instance, consumptive tubercular decay" (page 148). We do not believe that to deficient supply of food or to hereditary

^a Car toute la saine logique est reductible à cette seule règle, former toujours, la plus simple hypothèse compatible avec l'ensemble des renseignements obtenus.

taint, any more than to the breathing of prerespired air, are we entitled to attribute the production of tubercle to the exclusion of other influences. Referring to causation, Mr. J. S. Mill observes:—"It is seldom, if ever, between a consequent and a single antecedent, that this invariable sequence subsists." Consumption does not, in our opinion, form an exception to this doctrine, which finds so many illustrations both in morals and physics, and we cannot afford to reject any agent which has shown itself capable of preventing that *concurrence of conditions* which is likely to originate and maintain tubercular deposit. A philanthropic disposition, no less than vast erudition, is reflected from every page of Dr. MacCormac's book, and we commend it to the careful perusal of the profession.

Dr. Pollock's work is a contribution to the natural history of phthisis, for the study of which he has enjoyed very ample opportunities, which he appears to have laboriously improved. Prognosis involves not only a due appreciation of the circumstances under which a disease has arisen and which exert their influence on its progress, but, likewise, the estimate at their true value of the resources of art for its alleviation or cure, and hence the work before us contains much on the etiology and therapeutics of phthisis. Many of the views which Dr. Pollock holds do not differ from those which have guided the opinion of experienced physicians for years past, yet, by formularizing the results of his observations, he has made them available for the guidance of others who have not the impressions left on their mind by long practice to draw upon. In all questions into which comparison enters it is of course necessary to assume a standard, and this he does in "ordinary phthisis," the average duration of which he takes at a little more than fifteen months; but as his figures "represent merely the length of time for which the case had lasted from the earliest symptoms up to the latest period while under observation," the actual duration would be considerably longer. He, wisely we think, limits the term "premonitory stage" to that condition of emaciation, *malaise*, and fever, which, in his opinion, precedes the local mischief. On this point his views are somewhat at variance with those brought forward by his former colleague, Dr. E. Smith, in his work on the Early and Remediable Stage of Consumption. As is generally admitted, he shows that these symptoms possess a serious significance when they arise during convalescence from an acute disease, after an exhausting labour or prolonged lactation, or in a member of a phthisical family, but we doubt whether "the existence of the rheumatic diathesis, in

the individual or his parents" would generally be considered as lending probability to the diagnosis of phthisis in a doubtful case. In a special chapter he again insists on this connexion, and states that gout and tuberculosis are similarly allied, a conclusion which is certainly at variance with the opinion of Ancell, Walshe, and others.

Professor Laycock, who has done much to unravel diathetic states, particularly describes the peculiarities of the arthritic tubercular cachexia, and we can quite believe that when the rheumatic element is present in a phthisical patient it may give a peculiar character to the inflammation surrounding the deposit, and may render the disease less rapid, though, at the same time, less under the influence of remedies, and specially intolerant of cod-liver oil; but that the existence of the rheumatic diathesis in the individual or in his parents should invest with increased suspicion the symptoms of emaciation and pyrexia we cannot admit without further proof than is afforded by Dr. Pollock's statistics. He found rheumatism had preceded in 49, and succeeded to phthisis in 18, cases in a total of 4,530; so that if we take both together the connexion did not exist in more than one and a-half per cent. of his cases. The figures given in Dr. Cotton's lectures are even more adverse to the idea, as he had only six rheumatic cases in 1,000 phthisical.

The first departure from the course of ordinary consumption which the author notices is "acute phthisis," characterized by undeviating continuity of the morbid actions, and met with under two forms:—

1.—The *Acute Inflammatory*, in which the local disease is a combination of inflammatory products and of tubercle, as an example of which he quotes Dr. Law's case, recorded in the Proceedings of the Dublin Pathological Society, and published in the number of this Journal for May, 1864. Such cases are characterized by all or most of the following symptoms:—Rapid waste; continuous febrile disturbance with exacerbations; early gastric and intestinal derangement; marked hereditary taint, and in females, non-appearance or suppression of the catamenia—the physical signs are those of diffused deposit and of inflammation; they rapidly change, and those of all stages are found simultaneously in one or both lungs. The progress of such cases is entirely uncontrolled by remedies. The rapid softening of a limited deposit in one apex, or the sudden breaking up of an old one, may produce constitutional symptoms almost as alarming; but the history of the case and the limitation of the

physical signs will, with care, enable us to avoid so serious an error as mistaking such a case for one of acute inflammatory phthisis.

2.—A passive exudation of crude tubercle invading the whole of both lungs, with slight febrile disturbance. Of this he gives an interesting case. A delicate looking lad, with strong family tendency to phthisis, while spending the winter at Rome, began rather suddenly to lose flesh and strength without loss of appetite and without cough, expectoration, or hemoptysis; dulness on percussion, becoming gradually more marked, extended over the whole chest; the respiration at first deficient, was dry and tubular throughout, the voice bronchial. There was remarkable impairment of the mobility of the chest on both sides. Dyspnea on exertion was the only distressing symptom, and there were no secondary affections. Death took place within three months; and the entire extent of both lungs was found solidified by grey and yellow tubercles, without inflammatory products or softening; of such cases Dr. Pollock says:—"The prognosis is to be derived from its diagnostic features, which are peculiar and rare. The universality and rapidity of the deposit in both lungs, and the progressive waste of the system, the hereditary taint, and the absence of chest symptoms, and of acute fever, are the leading characters. I have never known such cases either to pause or recover."

Returning to ordinary phthisis, Dr. Pollock maintains that the disease may be arrested in its first stage, and the deposit absorbed, and gives the following summary of the conditions favourable to such a result.

"1. The nature of the tubercle deposited. If there be evidence from physical signs that we have a case of grey, scattered tubercles, few in number, as evident by alteration in the tone and duration of the respiratory sound and by slight dulness, we are to regard the case as favourable.

"2. The absence of signs of irritation in the lung, crepitant râles, much dulness, the various rhonchi, &c.

"3. Phthisis not inherited; the disease accidental;^a occurring in an individual of healthy antecedents; well-made chest; no previous syphilis nor courses of mercury.

"4. The constitutional disorder soon subsides. This is important; the disease may commence with very acute symptoms, which is not unfavourable, provided they decline early.

"5. One lung only affected; the right the most favourable.

^a Stokes: "All the cases which recovered, and they were not a few, were cases of accidental phthisis."—MS. Notes of Lectures.

“6. To these conditions must be added the possibility of the patient being placed in the best hygienic conditions, giving up all unwholesome occupation, and leading an open-air life, with liberal diet, &c.

“A concurrence of all of the above conditions is necessary to a favourable prognosis.”

The next most favourable result, if the deposit fail to be absorbed, is that it should be tolerated, and Dr. Pollock, in the eleventh chapter of his work, describes cases, seldom met with in persons under twenty or above forty-five years of age, in which a limited tubercular deposit receives no increase, and undergoes no change for an indefinite period; the most hopeful cases of this group are usually of the lymphatic temperament, without hereditary taint, with a fairly good digestion, and without the indications of local or general irritation, but there are other cases which, though claiming admission into the same group, hold their place there much less securely; those, namely, who are at intervals reminded by sub-febrile attacks and symptoms of pulmonary irritation that the disease, though generally quiescent, has not left its ground. In the twelfth chapter, Dr. Pollock gives us the result of his experience of cases in which cretaceous expectoration occurred, and which were generally slow in their progress. We may allude here to the treatment which, in one of the concluding chapters of his work, he recommends with reference to the special indications in the first stage:—

“Local depletion,” he says, “cannot be too strongly urged as the necessary, if not the sole, mode of relief in the early days of a declared deposit in the lung, with irritative fever.

“The common case of a florid hemoptysis, with high pulse, a daily febrile access and sweating, where dulness and feeble respiration, with prolonged expiration, are ascertained to exist at one apex, ought to be actively treated by local depletion, followed by counter-irritation. A few leeches followed by the cupping-glass over the seat of dulness, saline medicines, perfect repose, and a non-stimulant diet, offer the best chances of ensuring quiescence of the deposit which has already taken place. At this period it is vain to put in practice the system of feeding the blood which we have advocated in the premonitory stage. Our question is whether this local irritant shall proceed to destructive changes necessarily involving the lung, or be tranquillised into the condition of a foreign body tolerated in a vital structure. On this depend our hopes of chronicity, and the chances of absorption. Absorptive processes do not occur so long as local irritation remains. The changes for which we look in the most hopeful condition of deposited tubercle only take place when pulmonary irritation or

inflammation has ceased. Cod-liver oil and iron, with tonics, cannot control or immediately alter the deposit, nor prevent its remaining as a mechanical irritant. Neither can the stimulant and supporting treatment so commonly applied to every affection involving exhaustion of vital powers, be here available, or indeed be regarded as other than mischievous.

“When the local irritation has subsided, and the febrile symptoms lessen, the pulse sinking to a natural standard, and the digestive system becomes improved, as evidenced by a cleaning of the tongue, oil may be commenced with the best effects. Its use is perhaps best preceded by the lighter tonics, to which an alkali may with advantage be added.”

On the importance of availing ourselves of the first remission in the progress of the disease, Dr. Pollock makes most judicious observations.

“After such local irritation has subsided, we generally find that the system is in a condition to take up nutritive matter very actively. . . . It is remarkable with what ease the blood may be fed, and all the natural indications are for abundant nourishment and such assistance as we can afford by oil, iron, and tonics. We cannot speak too highly of the adjuvants of open air, full exercise, mental employment short of fatigue, the cold sponge bath, and an abundance of the more nutritive articles of food. By following this method, and by the use of prolonged counter-irritation, we have seen cases of the most threatening appearance become chronic, and in some instances the physical evidence of local disease disappear.”

The stage of softening, as well as the first stage of phthisis, may be very prolonged, and according to Dr. Pollock, we may hope that such will be the case when the surrounding congestion and irritation subside, the general health improves, the pauses in the progress of the disease become lengthened, and the sounds in the chest become dryer; he draws special attention to “Chronic Diffused Tubercle;” he gives it as the result of numerous observations, “that tubercle when diffused or spread throughout a large surface of one or both lungs undergoes its destructive changes slowly, and in certain instances dries up;” and that this condition is, within certain limits, favourable for chronicity in the first and second stages of the disease, but unfavourable in the stage of excavation; in the thirteenth chapter of the work will be found some interesting observations on the symptoms and physical signs of the form of phthisis, as well as on the circumstances which would in any individual case justify a favourable prognosis.

In the twenty-seventh chapter are found his directions for the

treatment of the second stage; they are those usually given in works on this subject. Contrary to the experience of Walshe and other observers, Dr. Pollock has found quinine sometimes useful against the hectic chill; and from iodide of potassium he has seen excellent results in certain cases of spasmodic dyspnea; when opiates are necessary, but derange digestion. "The best antidote is lemon juice or a small cup of strong coffee taken on waking." Dr. Pollock then proceeds to show that the stage which is the last in the series of pathological changes, may likewise be very greatly prolonged; while the cavity is forming, the position of the patient is sometimes critical; but once this has actually taken place, local irritation and constitutional suffering may both subside; the circumstances which of course mainly influence the result are the intensity of the constitutional predisposition, the external circumstances of the patient, the degree to which the local mischief has extended and the amount of conservative energy in the organs which maintain innervation and digestion; for the details, we must refer to the work itself, where a very elaborate table of the prognostics is given.

Several pages are devoted to digital clubbing, which Dr. Pollock has found associated almost exclusively with that form of phthisis in which, though more than one cavity has formed, toleration of the disease may be expected; on the treatment specially required in this, the third stage, some remarks will also be found towards the close of the volume.

A chapter is devoted to phthisis accompanied by external struma. He has found some peculiarities in its localisation and progress, which appears to have been somewhat slower than ordinary phthisis. In persons above forty-five years of age consumption is, on the whole, slow; but Dr. Pollock adds the weight of his experience to the opinion of those who consider cod-liver oil is greatly less beneficial in the aged, and travel is not, he thinks, to be recommended to those who have passed sixty. As is now generally held, he views any attempt to heal fistula in ano in the consumptive, as most injudicious; and believes that whatever temporary power pregnancy may have in rendering the disease quiescent, labour and suckling generally lend to it a fatal rapidity.

The chapter on Pneumothorax contains some interesting observations; as do those on Hemoptysis, Hereditary Transmission, and the Influence of Occupation on the Phthisical; though on these subjects Dr. Pollock's views are simply confirmatory of those usually held.

Under the head of Preventive Treatment of Phthisis in Infancy

and Childhood, Dr. Pollock gives much judicious advice as to the best means of warding off the disease from those who inherit a tendency to it. He dwells on the necessity for good food, the danger of lowering treatment when they are attacked by illness, the importance of air, light, bathing, and suitable clothing, the imprudence of over-taxing their powers, either mental or bodily, the evil results arising from the abuse of stimulants and tobacco, and the importance of selecting a profession favourable to health. Those practitioners who take up Dr. Pollock's book to gain, from a hasty perusal of this chapter, some suggestions for their assistance in such cases, would, we think, be led into an erroneous practice in one respect. At page 355 he recommends that the children of those who can afford to go abroad, should spend their Winters in one of the health-resorts of Italy or France, which permit of horse exercise at that period of the year. From some remarks elsewhere in the work, we are disposed to think the author's views on this subject are not very different to our own; but as the paragraph stands it would lead others to give advice from the adoption of which evil instead of good would, we believe, generally follow. The benefit to be derived from change of air and scene may occasionally render a Winter tour in Southern Europe desirable, but as a rule, we are convinced that at this stage, when we have to deal not with a local disease but with a condition of low vital action, it is a positive and great injury to a youth to lose the more bracing effects of our northern Winter, the vicissitudes of which may, with ordinary caution, prove of real service to him. Those youths who, *fruges consumere nati*, can live where they like, are more likely to find vigour and strength, while they seek only amusement, on the moors or in the hunting field at home, than in the *conscious* pursuit of health over the weary waste of the Campagna, or killing time, as best they can, among the acknowledged valetudinarians of Pau or Mentone.

The chapter on the general, or as it is termed the direct treatment of phthisis, may be read with advantage by those who are still disposed to trust to drugs, to the neglect of open air exercise, gymnastics, and healthy sports.

On the subject of climate, to which is devoted the last chapter of Dr. Pollock's work, we shall offer a few remarks in connexion with the book which is next on our list.

The introductory chapters, which we have purposely postponed noticing, contain general considerations on chronic organic disease,

and on the resistance of the system; on the mode in which the *vis vitæ conservandæ* shows itself by compensatory arrangements and efforts to limit disease, a subject of great interest and practical importance, but requiring, as every attempt at generalization does, a great accumulation of facts collected in different departments of observation, and the rare power of keeping these clearly before the mind; and we think Dr. Pollock has rather argued from the particular, with which his inquiries have made him most familiar, to the universal. There is one pet theory he has got, which, after the reader thinks he has discarded it for the ordinary view, crops up again towards the close of the book, and we feel constrained to notice it because we think it not only fanciful but liable to mislead. At page 20 we find the following sentence:—"In tracing the analogy between the natural processes in a pneumonia and in a deposit of tubercle, as indicative in both of a conservative energy on the part of the system, it must be remarked that in both the febrile disturbance seems to have a common object, viz., an abatement of the requirements of the blood for air, and the necessary reduction of the tissues;" and at page 23 the author thus expresses himself:—"Nor are the systemic changes less characteristic of resistance to the progress of chronic morbid changes than the local. The reduction of the solids of the body, the emaciation, muscular waste, and absorption of fat, are necessary (as already stated) in order to establish a balance between the system as a whole and the organs which are to supply it." The true nature of pneumonia is still *sub judice*. Competent pathologists are not agreed as to whether the pyrexia has its analogue in the fever which follows a local injury or the pulmonary lesion, its counterpart in the articular inflammation, in the production of which the blood-poison of gout expends itself; but we do not see that Dr. Pollock's idea of the physiological import of emaciation would fit into either theory. In phthisis emaciation often precedes, by a considerable interval, the local mischief; and even if it did not, we cannot conceive that nature means to lessen the labour of the oxygenating organs by loading the blood with the hydro-carbonaceous products of waste.

In a monograph on pulmonary tuberculosis we were surprised to find no mention made of the peculiar form of dyspepsia which so frequently accompanies the cachexy, nor of the alterations in the temperature of the body, which have been shown to mark the commencement and increase of the deposit.

The question of change of climate for a consumptive invalid is

one of the most anxious which a medical adviser has to determine—a resource which undoubtedly does accomplish what no other means will, should not lightly be neglected, but a step which in unsuitable cases hastens death, and deprives the sufferer, in the last stage of the complaint, of the comforts of home, must not be recommended without serious consideration. Formerly it was a hope to which the wealthy alone could cling, but now in our Australian colonies the poor man may seek to better alike his health and his prospects. We are convinced Mr. Brown's little work will prove useful. It reminds us of the fact which every one knew, but which many did not sufficiently consider, that it is a matter of no small importance, as far as regards health, for the invalid to make a careful selection among the various climates of Australia.

“Tasmania,” he says, “may be considered *the climate par excellence*, not only of the Antipodes, but, I believe, of the universe, for that class of the English consumptive invalids who require a climate more equable than our own, but not of much greater temperature, and with an ozonized, clear, and bracing atmosphere.” “The neighbourhood of Hobart Town, and the mountainous districts, is to be preferred to that north and north-west of the island, where, as in Launceston, the land is lower and in some places marshy. New South Wales is adapted to those cases which require a temperature warmer than the one in which the disease was manifested; and, as a Winter or Spring climate offers, in combination with residence in Tasmania (during the Summer months), many advantages—Sydney, however, is the least agreeable portion of this colony—Goulburn, Bathurst, the Illawarra, and Hunter districts, each present to the traveller various climates, scenery, and occupations.”

Mr. Brown advises invalids to go to Melbourne in a comfortable sailing ship, and hence by the inter-colonial steamers. Having had some experience in such matters ourselves, we can say that the invalid bound for Australia will find in his work many valuable hints, by attention to which the comfort and good effects of the voyage will be much increased. His observations on Australian prospects may be read with advantage by others. The book is intended for the laity, and does not, therefore, contain much information on the conditions which should lead us to advise change of climate. On this subject we consider Dr. Pollock's remarks very just. He shows that the cases of phthisis benefited by change go away when the irritive fever is absent, the local disease not

advancing, and the digestion tolerably healthy; that the Spring, and not the Winter, is the dangerous season for England, and that a pure dry air of moderate temperature is the one to be sought for. Both he and Mr. Brown strongly urge a long sea voyage as of inestimable benefit in many cases, an opinion in which we fully concur.

“Emphysema, its Pathology—and Bronchitis, its Treatment,” or some similar title, would have conveyed to his professional brethren a more correct idea of the scope of Dr. Dobell’s work than the one which his modesty, no doubt, has led him to select. WINTER COUGH, however, may lead a larger number, if not a more discriminating class, to interest themselves in the book and its author. Rejecting the theory of disease of the walls of the air cells, and also that of compensatory distention during inspiration, as explanations of the mechanism of emphysema, Dr. Dobell follows Dr. Jenner and others in believing that the injurious pressure is exercised on the vesicles during expiration; and having shown that this pressure comes to be exercised owing to obstruction to the outward current, he draws attention to some hitherto unnoticed conditions, which are liable to give rise to this obstruction—nasal and post-nasal catarrh, catarrhal thickening of the mucous lining of the larynx, trachea, and large bronchi. As everyone knows, however, repeated attacks of bronchitis, more frequently than all other causes, lay the foundation of emphysema; and under the head of treatment, Dr. Dobell insists on the necessity of curing bronchitis completely, and taking precautions against its return. The book has the merit of being clearly written; and those who may not agree with the author’s conclusions will have no trouble in following his reasoning, and may get useful suggestions from his chapters on treatment.

Laryngoskopiske Iagttagelser hos Døvstumme. Af Dr. L. W. Salomonsen.

Laryngoscopical Observations on the Deaf and Dumb. By Dr. L. W. Salomonsen. Reprinted from the *Bibliothek for Læger*, Jan., 1866. 8vo, pp. 14.

THE author was led, partly by a statement by Dr. Gibb, that in examining two deaf mutes he had found that the chordæ vocales were wanting, partly by the interest he took in the efforts to teach

the deaf and dumb to speak, to undertake the laryngoscopical examination of a number of such individuals.

“As is well known, there are in Copenhagen two institutions for the deaf and dumb. In one—the Royal—so far as I am aware, no other language than that of signs and fingers is employed; no effort is made to teach the pupils vocal language: it is, or at least it was formerly, rather difficult for strangers, even for physicians, to obtain access to the institution. In the other—the private or deaf-mute school—based upon the speaking method, on the contrary, a considerable number (from 70 to 80) of pupils are instructed in and by the vocal method, and the institution is open to every visitor; the manager, Hr. Emil Keller, receives everyone in the most obliging and polite manner, for which I, for my part, return him my sincere thanks. This is not the place to enter into a discussion respecting the advantages and the defects of the vocal method, or to give any opinion as to the efficiency of this school; but this much I may be permitted to say, that although in my mind it does not come up to the ideal of such a school, and cannot, for example, be compared to the deaf-mute institution in Vienna, its whole plan and arrangement are so excellent, and its results so good, that it fully deserves the favourable judgment and the general and steadily-increasing sympathy on which it has hitherto had to congratulate itself. I sought and got permission to make laryngoscopical investigations on the pupils of this school, my object being to see how far the circumstances mentioned by Gibb should prove to be more general, in which case vocal language would for many be, of course, a physical impossibility; or whether, on the other hand, in accordance with the opinion hitherto usually entertained, the vocal organs should appear to be normal, the essential cause of the deaf-mutism being the deafness, while vocal language would not only have the natural physical condition for its possibility, but would also present itself as that which ought to be aimed at. I shall not delay to describe all the difficulties, on the patients' side, with which I had to contend in endeavouring to carry out the laryngoscopical examination; anyone can imagine how much the patients' want of hearing, the absence of control over the tongue, their fright, their uncertainty as to the directions given, &c., &c., must interfere with our efforts, and how much time must often be consumed in preliminary exercises. I shall only take leave to mention the, so far as I can judge, very satisfactory results I have obtained.”—Pp. 123-125.

All the examinations in general exhibited one and the same perfect or more or less normal condition of the vocal organs. Dr. Salomonsen lays the results before his readers in a table which

occupies upwards of six pages of his very interesting essay. Upon these results he makes the following remarks:—

“This table accordingly shows as follows:—Of the 70 or 80 patients in the school 24, or about one-third were examined; they were taken at random, for which reason their ages varied from 9 to 19 years, but the majority were, nevertheless, from the upper classes, and had been a considerable time in the school; the pupils in the lowest class were too small and too nervous to admit of their being examined. Among these 24 there were two who could not be examined with the laryngoscope; in 5 the epiglottis was so strongly directed backwards that it entirely, or at all events essentially, concealed the deeper parts and prevented the accurate investigation of the latter; in 17 all the deeper parts were seen and were found to be all normal; in 14 the chordæ vocales closed even quite to one another in the pronunciation of ‘æ,’ and only in 3 did a little elliptical slit remain between them. It will be readily granted, that if the subjects examined had been sound individuals, accurate laryngoscopy could have been carried out in more than 17 among 24; but from the remaining 7 we must, in the first place, subtract the 2 in whom the tonsils prevented the examination; and, in the second place, I have no doubt that in the five in whom the direction of the epiglottis made the investigation difficult, I should nevertheless have been able to accomplish it if they had been hearing and speaking persons, or if I had several times practised them in bearing the instrument. The main result, however, stands good; the doubt and reserve which one involuntarily feels in reading Gibb’s statement that the chordæ vocales were deficient in the only two deaf-mutes he casually examined, and which Semeleder, for example, has given expression to by placing a note of interrogation after this statement; this doubt must be much strengthened, nay, almost followed by the conviction that Gibb did not know how to overcome the technical difficulties in laryngoscopising these persons, when we see that in the overwhelming majority, not to say in all the deaf-mutes examined by me, not only were all the constituent parts of the vocal organ present and completely normal, but also that the chordæ vocales in uttering a sound perfectly executed the normal movements. In these facts lies not only an incontestable confirmation of the correctness of the original popular assumption that it is essentially the deafness which causes, and carries with it the dumbness; but there is contained in them also, as it appears to me, a weighty argument in favour of the introduction of the vocal method in the instruction of deaf-mutes. . . . But I foresee an objection to the demonstrative force of these investigations—namely, that among the 24 deaf-mutes the deaf-mutism was congenital in only 6; in all the others it was acquired. But, in fact, this objection does not carry much weight; for in the first place we must to these six

add two, in whose cases the age at which the deaf-mutism occurred is unknown. In the next place there are among the others several, at least six, in whom the deaf-mutism is stated to have commenced between two and five years of age, consequently before there could have been any idea of their learning to write; and as to them it is a generally-admitted proposition that the deaf-mutism, with respect to the difficulty of learning to speak, may be placed on a level with congenital deaf-mutism; and lastly, it is certain that similar circumstances are to be met with in every collection of deaf-mutes. . . . Thus Mitchell^a states that among deaf-mutes the deaf-mutism is not congenital, but is acquired after birth: in Great Britain, in 25 per cent.; in the United States of North America, in 42 per cent.; in Germany, in 52 per cent. Among my 24 individuals it arose, after birth, in 18, or perhaps more correctly, in only 16, that is, in 66 per cent. The difference between this and the proportion in Germany is not greater than might depend upon accidental circumstances, and at all events cannot afford any firmer support to the objection above referred to."—pp. 132-134.

We have shown our sense of the value and importance of Dr. Salomonsen's observations by the length at which we have quoted from them. On one point just alluded to we must, however, take exception to his remarks, namely, where he places deaf-mutes meeting with their affliction at from two to five years of age on a par with congenital deaf-mutes in respect to learning to speak. There can be no doubt that a child who should reach even two years before becoming deaf, would possess a very great advantage in its subsequent education over one born deaf. We were ourselves acquainted with a young gentleman who totally lost his hearing, from fever, at five years of age, but who up to the period of his death, from phthisis, twenty-two years later, could speak so as to be easily understood, while his friends, in communicating with him, were obliged either to use their fingers, or to articulate so distinctly that he could, from the motions of the lips, judge what was said. In this latter mode he was able even, if favourably placed, to follow *some* preachers so perfectly, as to take in the meaning of their sermons.

^a Annales d'hygiène publique et de médecine légale. July, 1865, p. 77.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.^a

DR. M'DOWEL, President.

Scirrhus Tumour in the Mammary Region.—MR. TUFNELL said he brought forward the specimen now before the society, as a case not of cancer of the breast, but as a cancerous tumour in the breast, which he thought would be interesting to surgeons, with regard to the question of removing the breast itself in cases of malignant tumours. The individual from whom he had taken the morbid specimen was a woman fifty years of age, and in the rudest health, being apparently not more than thirty years old. About August last she fell against a hard body, and gave the breast a severe blow. Early in November she first noticed, below the left clavicle, a tumour about the size of a pea. This gradually increased until it attained its present size, viz., that of a small apple. It was a densely scirrroid body. The mamma itself was perfect, the tumour separated from it entirely, growing from above downwards, and from without inwards, until it at last became adherent to the cartilage of one of the ribs; its base was polished as if lined with a serous membrane. The tumour was so hard that it had quite the feeling of an unripe pear. Under the microscope it gave the ordinary characters of scirrhus. Mr. Tufnell presented it to show that it was a tumour in the breast, but not a cancer of the mammary gland, which was perfectly healthy. It was a case which proves that although a cancer might be in the vicinity of the breast, it was not always necessary to remove the mamma itself, as some surgeons maintained, and carried out in practice.—*December 9, 1865.*

Epulis.—DR. FLEMING exhibited a cast and drawing illustrative of the appearances present in a case of epulis in a boy aged between five and six years. The disease occupied the left side of the lower jaw, in the interval between the first and second molar teeth both of which were displaced and inbedded in the morbid growth

^a These reports are furnished by Dr. W. Smith, Secretary to the Society.

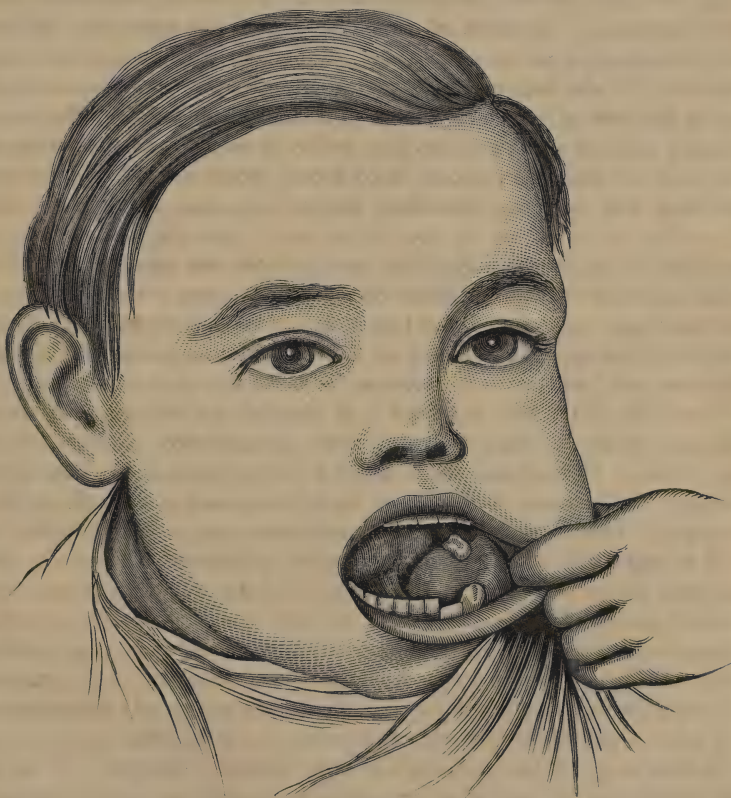
The history of the commencement of the disease was unsatisfactory, as well on account of the age of the child as of the fact that the mother had only accidentally discovered it about a month back, when, perceiving a fulness in the cheek of the child, and examining the mouth, a tumour attracted her attention. The child complained of no uneasiness from the affection; he eat and drank as the other children, neither was articulation remarkably affected. She never observed any bleeding or any discharge of any kind from the mouth, and the appearance and the condition of the child indicated ordinary good health. The drawing, by Mr. Connolly, and the cast, for which Dr. Fleming was indebted to Mr. Grimshaw, gave a very accurate representation of the locality and of the appearance of the morbid growth. It occupied the space between the first deciduous or temporary molar tooth and the first permanent molar. The second deciduous molar was not visible. The physical characters of the mass were much as those of the ordinary gum structure—the colour and the consistence being identical; in size and outline it much resembled a small tomato fruit, the under part of which would be as if sulcated to embrace the corresponding alveolar portion of the jaw, whilst its lateral portion overlapped the inner and outer projections; the different surfaces of the tumour were nodulated and irregular, the outer bulging less than the inner, which encroached so much on the floor of the mouth as to displace the tongue, and push it up to the right side, and towards the hard palate. The covering of the tumour was unbroken over the mucous membrane; it was devoid of pain on pressure, and free from any trace of ulceration. There had never been any hemorrhage from it. The teeth contiguous were slightly displaced and loosened in the sockets, but otherwise they were sound.

When the child was admitted into hospital, he had an eczematous eruption about the face, which delayed any operative interference for some time. During this there was not any appreciable increase in the tumour. Its removal was decided upon, commencing with that of the contiguous teeth. A knife, shaped somewhat like a farrier's draw or hoof-paring knife, was used for the purpose with great advantage. And this was done on a level with the inside of the adjoining gum. The alveolar portion between the teeth removed, was freely excised with a strong-backed scalpel. Sharp hemorrhage followed, which was quickly controlled by ice and ordinary astringent applications. In the process of healing, some portions of the alveolar sockets exfoliated; and when the boy left hospital there was every indication of a satisfactory cure.

A knowledge of the pathology of this disease is indispensable to its radical extirpation; and when we reflect on its anatomy, involving as it does the gum structure, the periosteum, and the periodontal membrane, much care is required. The peculiarities in this case are, the apparently rapid growth of the disease, its engaging both lateral aspects of the body of the jaw, and perhaps its special locality. None of the symptoms usually attendant on malignant disease were present.

Through the kindness of Dr. Barton, Dr. Fleming was enabled to exhibit a drawing of a case which occurred under his care, in a little girl aged three years, where an epuloid growth existed, which was successfully removed, but where the external features of the disease had an apparently different character. The colour of the new growth was very peculiar as contrasted with that in the case now exhibited to the society, and the displacement of the teeth was greater. Here the teeth were removed, and also the adjoining portions of the alveoli. It appeared to Dr. Fleming to belong to that form of the disease which is termed myeloid.

The accompanying woodcut, by Mr. Oldham, represents accurately the morbid growth.—*December 9, 1865.*



Malignant Tumour.—MR. CROLY exhibited a tumour which he had removed from the neck of a woman in the City of Dublin Hospital on the previous Thursday. The patient, forty years of age, was of sallow com-

plexion, but had in general enjoyed good health. The disease commenced to grow between two and three months previously. It soon became as large as a tennis ball, was never very painful, but occasionally she felt a sting down her arm. She could not lie comfortably on her right side as symptoms of suffocation were produced by that posture.

The tumour quickly increased in size, and, on admission into Hospital, was as large as the section of a small orange, convex and even on the surface, situated beneath the ramus of the right side of the lower maxilla, extending upwards near the mastoid process, downwards beneath the angle of the jaw, backwards close to the spinous processes of the cervical vertebræ, and downwards and forwards to the submaxillary region; it had a fluctuating feel and was partly movable. On consultation, it was decided that the growth should be removed, which was accordingly done by Mr. Croly. The necessary dissection was difficult, and exposed the sheath of the carotid artery; the diseased mass was removed with the fingers, and a portion which extended deeply backwards to the transverse processes of the vertebræ was ligatured. The microscopic examination of the morbid structure showed a soft fatty material like marrow, and numerous nucleated cells. There could be no doubt of its being of the nature of medullary cancer.—*December 16, 1865.*

Mitral Valve Disease ; Pericarditis.—Dr. LAW said the morbid specimens which he now submitted to the society, was a complication of pericarditis, mitral valve disease admitting of free regurgitation and extensive hypertrophy. The subject of the case was a boy twelve years old. About a year previously he had consulted him for headache and palpitation of the heart. The affection of the heart then appeared to be enlarged mitral orifice, admitting of free regurgitation. The grounds of his diagnosis were these: the abnormal sound heard in the situation of the apex of the heart, externally on the left side, and posteriorly between scapulæ. There was also extensive dulness on percussion in the cardiac region, especially in the transverse direction. These symptoms he thought conclusive as to the nature of the lesion; the abnormal sound, the increased dulness on percussion, and the strong action of the heart. The boy complained of headache which Dr. Law had no doubt was a mere rheumatic headache; for he had originally had rheumatism, and upon that followed the affection of the heart. When he came under his care last, on the 12th of May, what he then chiefly complained of was distress in his breathing and general discomfort, not actual pain. His pulse was about 132, there was not much heat of skin, but there were the same cardiac signs that existed before, namely, an abnormal sound in the situation of the apex of the heart, heard extensively over the side, and posteriorly. At the same time he had pain in his ankles, but without swelling or redness. These pains yielded to stuping. He complained still of

considerable distress about the cardiac region. First of all Dr. Law only heard strong, quick, and rather tumultuous action of the heart. On the third day of his admission he discovered a friction sound in the region of the heart. This sound became more distinct and more extended day after day; and each day, for a period of ten, they had an opportunity of hearing, all through the cardiac region, different modifications of the friction sound, sometimes coarser, sometimes finer; but for ten days the friction sound was never absent, varying in different situations. There was fulness in the cardiac region, and a distinct vermicular motion perceptible. After ten days the friction sound entirely disappeared. While present it entirely masked anteriorly the abnormal sound that indicated the valvular lesion; but this was always heard posteriorly. When the friction sound disappeared, there was a strong action of the heart, from which Dr. Law considered that adhesion had taken place, and he regarded this increased action of the organ as an effort to relieve itself of this impediment to its action. On the second day after the cessation of the friction sound, the symptoms underwent a very great aggravation. His dyspnea was greater, his face became puffed, his legs swelled, and general dropsy set in. His appetite failed, and he went on from bad to worse, and so expired. His illness altogether occupied about three weeks. He was disposed to consider this aggravation of the symptoms to depend on the impediment to the heart's action in consequence of the adhesion which he expected to find.

The *post mortem* examination exactly confirmed the diagnosis in every particular. The heart when taken out weighed twenty-eight and a half ounces; the average weight of the adult heart being only nine. The cavity of the pericardium was completely obliterated, in parts its sides admitted of being separated, and the whole organ was found coated with lymph varying in depth and in density, and Dr. Law thought he could directly connect the different appearances of the lymph with the coarser or finer *frottement* existing during life; towards the apex, the lymph being finer, showed there was more motion and therefore the more friction; and towards the base less fine from there being less motion and consequently less friction of the surfaces against each other. Immediately under the lymph, and closely applied to the heart, there was a dense fibrous substratum indicating successive attacks of pericarditis. It was exactly what Rokitanski remarked when he described the textural development that fibrin, the result of inflammation, undergoes, that it becomes the seat of chronic inflammation which consists of a linked succession, stage for stage, of inflammations, more or less acute; amongst the most remarkable of which were those occurring in the vascularized product of an antecedent inflammatory process, more particularly in the pseudo-membranous products of inflammation on serous membranes. Dr. Law remarked that he had often seen exhibited at their meetings, specimens of

what were called hemorrhagic pericarditis, but which from the depth of the false membrane, the seat of the hemorrhage, and its density, must have had an origin long anterior to the present inflammation. He remembered that some time since Dr. Byrne exhibited a case of hemorrhagic pericarditis, in which there was recent lymph with some blood and a substratum of fat. He was satisfied at the time that in this case there had been a succession of inflammations.

As to the condition of the mitral orifice, it was very much enlarged, so as easily to admit of three fingers being passed into it; nor were they left to conjecture as to the course of the blood when it passed in greatest quantity; for on looking at the aorta they saw how small it was compared with the bulk of the heart. It was a physiological principle that a vessel accommodated itself to the amount of blood that passed through it, and they were justified in believing the blood in this instance flowed freely back. They had an opportunity of seeing the peculiar modification of shape produced by this lesion, differing from that which it undergoes in aortic valve disease; in the latter we had an elongated heart; here we had it hypertrophied in a transverse direction. This, too, confirmed the observation as to the direction the blood took. As in one case hypertrophy was established to sustain the returning column of blood; here on the contrary it was directed against the mass of this fluid, coming from the auricle into the ventricle before its time, so that there was a collision, as it were, between the blood coming from the auricle into the ventricle, and that passing back or regurgitating into the auricle. This explained why the heart was enlarged in the transverse direction in mitral valve disease, and elongated in cases of disease of the aortic valves. This boy was the member of a family consisting of five children, and three of them were at this moment labouring under heart disease; while neither parent was rheumatic.—*December 16, 1865.*

Varicose Aneurism.—DR. HAYDEN brought before the society an example of spontaneous varicose aneurism, engaging the aorta and the superior vena cava. The patient was a man thirty-three years of age, a mason by trade, and of intemperate habits. He had enjoyed excellent health up to the last year of his life. During that time he had suffered considerable privations, but still he did not complain of positive illness. About five weeks before Dr. Hayden saw him, it appeared that whilst lifting a heavy stone from off the shoulders of a fellow tradesman he made a false step and strained his back. He complained of acute pain. at the moment, in the dorsal region, and in consequence was unable to work for some days, but then resumed his labour and went on as usual. A fortnight subsequently, while walking in the street, he experienced a sudden sensation of weakness in the lower limbs, and was about to fall, but supported himself: the feeling of faintness passed off in a few minutes, and he was able to proceed.

About this time he called at the Mater Misericordiæ Hospital and was seen by Dr. Hayden : he then seemed to be under the influence of drink. He coughed slightly and was hoarse. He did not think it necessary on that occasion to make an examination of the chest. Three weeks afterwards he again presented himself at the hospital. His condition was then so striking that Dr. Hayden at once admitted him, and after placing him in bed made a careful examination of the chest. The face, neck, and chest were deeply cyanosed, tinged, and œdematous : the face and neck in particular were of a deep purple tint ; the conjunctivæ were injected with dark, venous blood ; the pupils contracted, but equally so : ears and lips of a deep, leaden hue, and the neck bloated, so that the angles of the inferior maxilla and the supra-clavicular fossæ were obliterated. The cervical veins were distended, and yielded a doughy feel. They were, however, devoid of pulsation and thrill. The chest was covered throughout with groups of tinged and tortuous subcutaneous veins : those of the arms and forearms were also distended, and the finger ends of a dark, livid colour. From the diaphragm downwards the body was of the normal colour and temperature ; above this line the surface was remarkably cold, and the man constantly complained of a feeling of chill ; but in the lower part of the body he had no such sensation, and the feet were of the normal temperature. There was cough and expectoration. The pulse was 114—small, but regular ; the respiration, 24. The tongue was moist ; the bowels natural. There was no dysphagia.

On carefully examining the chest he found it unusually resonant ; a little less so to the right of the sternum, and from the clavicle down to the nipple. Respiration was remarkably loud over the entire of the left side of the chest, both before and behind, on both sides. It was accompanied by muco-crepitant râle. Over the lower portion of the right side respiration was likewise normal ; but above the nipple, on that side, it was feeble, as it was also from the middle of the infra-spinous fossa to the apex of the chest.

There was increased extent of precordial dulness ; the heart's action was regular ; the impulse strong, but unaccompanied with *fremissement* ; the apex pulsated behind the ensiform cartilage. Over the base of the heart was heard a double *bruit de soufflet* ; the first or systolic murmur was coarse, loud, and transmitted upwards in the course of the aorta, through its arch and into the carotids ; it had its point of maximum intensity at the junction of the third right costal cartilage with the sternum, and was, moreover, diffused over the entire chest anteriorly, but louder on the right side ; it was likewise audible in the right infra-spinous fossa.

The second murmur, audible over the lower portion of the sternum, was diastolic, and seemed to replace the second sound of the heart ; it was soft and somewhat masked by the former murmur, and strictly

confined to the region of the base. On placing one hand upon his chest in front, and another behind, and desiring him to respire forcibly whilst the hands were in this position, a feeble impulse was felt in the upper portion anteriorly, at the *acme of expiration*.

The condition of the patient remained somewhat the same for several days. On the night of the 20th of October, three days after his admission into hospital, he was slightly incoherent, and on the following morning he (Dr. H.) thought it prudent to extract some blood from the engorged veins. He accordingly had nine leeches applied to the upper part of the chest. The leech-bites bled freely, two of them so much so that it became necessary to apply caustic to arrest the hemorrhage.

On the 22nd of October the man complained of some slight difficulty in swallowing. This difficulty, however, was traceable to the state of tension of the integuments of the upper part of the neck.

On the 23rd the state of engorgement of the veins of the upper part of the body, and the tension of the arms were so great, that he thought it prudent to take some blood from the arm. The tension of the veins of the forearms was such that the application of the ordinary bandage was unnecessary. About eight ounces of blood were taken from him, with the effect of giving him much relief. On the 24th the superficial veins of the left forearm were in a state of phlebitis; they were hard, tender to pressure, and marked by lines of ecchymosis. Six leeches were applied here, followed by warm poultices. The leech-bites bled freely and the man experienced relief. At this time his colleagues and the class were struck forcibly with the appearance of the man. His condition remained little altered till the 27th of October. In the interval an attempt was made to have his photograph taken, but to this the man evinced an unconquerable objection, and the idea had to be abandoned. On the 27th of October he complained of an overpowering sensation of drowsiness, and shortly afterwards died without a struggle.

The chest only was examined. On laying open the thorax it was found that the lungs had completely enfolded the pericardium and were adherent to it. They were engorged with blood and serum, which flowed freely from them on section. They floated in water. The pericardium was greatly thickened and adherent to the heart throughout. The heart was of the normal size and consistence. The right auricle contained a mass of dark, coagulated blood: the superior cava, at its junction with the auricle, was flattened from before backwards, and its two walls attached to one another in the centre. At one side of this adhesion there was a passage into the auricle through which the little finger might be freely introduced, but on the other side of the abnormal septum the opening was greatly contracted, admitting only of the introduction of an ordinary-sized probe. He was at first under the impression that this adhesion was caused by inflammation; but Professor

R. W. Smith, who examined the specimen carefully, had expressed his opinion that it was congenital. The ventricles were normal; the trunk of the pulmonary artery and its valves were healthy, as were likewise the left auricle and ventricle, and the mitral and aortic valves. The aorta was considerably dilated, and its lining membrane dotted over with pale yellow spots.

In the posterior wall of this vessel, and about half an inch above the valves, was a large opening of an ovoid figure. This opening extended across the artery, and measured, transversely, an inch and three-quarters, and vertically one inch. The margins of this aperture were smooth, but still irregular. It led into a large aneurismal sac, large enough to contain a moderate-sized apple. The walls of this sac were in a rugous condition, but composed of the coats of the aorta, the lining membrane of which was traceable into it. The sac contained a few loose masses of fibrin and some dark coagulated blood. This tumour had pressed backwards on the right branch of the pulmonary artery, which it had almost obliterated. It had pressed forwards and outwards upon the vena cava descendens, and occluded that vessel from its entrance into the auricle to a point half an inch below the junction of the vena azygos, and formed a communication with the cava by three openings in that vessel, one of which was the size of a three-penny piece, whilst the others were much smaller. The edges of these openings were thin, jagged, and bordered by red areolæ on the lining membrane of the vein.

The aneurism had likewise pressed backwards and upwards upon the right bronchus, upon which, when distended by the contraction of the left ventricle, it must have made intermittent pressure; hence the feeble character of the respiration on the right side.

This case seemed to him to be of considerable interest in many points of view. In the first place, the physical evidence for a positive diagnosis was imperfect. The diagnosis of aneurism was made rather from the general symptoms and history of the case than from physical evidence. There could be no doubt, from the engorgement of the superior vena cava and its tributaries, that pressure was exercised upon this vessel. This pressure was manifestly due to a tumour, which likewise, in all probability, in the absence of evidence of disease of the right lung to explain the feeble character of the respiration on that side, made pressure on the right bronchus. From the history of the case, as well as from the physical signs such as they were, there could be little doubt that this tumour was aneurismal. Thus there was a double bruit at the base of the heart. One of these became more intense as it was traced upwards, and at the right of the mesial line of the sternum it attained its maximum of intensity. At this situation, likewise at the acme of expiration, there was a very feeble impulse and thrill experienced. The physical signs, though not of a very decisive character, went strongly to confirm the

diagnosis of aneurism of the aorta. The absence of well-marked impulse, positive dulness, and dysphagia, was explained by the position of the aneurism with relation to the lungs and œsophagus.

There were several cases of this kind reported in the *Proceedings of the Pathological Society*.

The case which most closely resembled that now before the society was exhibited and described in 1842 by Professor Law. As in the present case, the physical evidence was very imperfect. There was no impulse or fremitus. Respiration afforded but little evidence. There was no dulness on percussion. In that case the tumour had formed on the posterior wall of the aorta and found entrance into the cava about an inch above the connexion of that vessel with the auricle. The appearance of the patient in that case was very like that of the man of whom he (Dr. Hayden) was now speaking. Most of the members present would recollect the remarkable case reported by the late Dr. Morgan in the November number of the *Dub. Quart. Journal* for 1853. The tint of the skin in that case was different from its colour in the present instance, being of a red crimson hue.

The interest of this case was considerable in other respects. For example, there was a double bruit over the base of the heart without aortic valve disease, this bruit being permanent. Hitherto it had been held by the Dublin school that a permanent double murmur over the base of the heart should be accepted as evidence of aortic valve disease; but here was a case of double bruit where the aortic valves were healthy. It has been conjectured that the growth of aortic aneurism arising within the pericardium as being due mainly to the reflux of blood in that vessel, is more likely to be in the direction of the heart, of which Professor Smith has recorded a remarkable example in the ninth volume of the *Dub. Quart. Journal*. To this rule the present case, as well as that of Dr. Law, constitute exceptions.

In the cases recorded by Drs. Law and Mayne the pressure on the cava was made above the vena azygos. Here it was below it; and this must make a difference as regards the state of turgescence of the vessels. Here the azygos, being pervious, was capable of carrying on the circulation to a certain extent, and was therefore greatly enlarged; whereas in those cases where pressure was exerted above the azygos, it could not aid in carrying on the circulation save by collateral channels of communication from the internal mammary and intercostal veins.—*December 16, 1865.*

Cystic Disease of the Kidneys.—Dr. G. F. DUFFEY, Assistant-Surgeon 1st Battalion 24th Regiment, detailed the following remarkable case, and exhibited the specimen and an illustrative drawing:—

On the morning of the 3rd of November, 1865, I was asked to visit a

soldier's wife who was in labour. She was twenty-two years of age; this was her third pregnancy. The first child was living; but in the second pregnancy she had a miscarriage. The waters, which were not excessive in quantity, had come away during the night. On examination I found that the feet were presenting through the external orifice, with the toes towards the pubes, and the left leg a little in advance of its fellow. She had comparatively frequent but short pains. The bowels had been opened and the bladder emptied. Little progress being made, the uterus was excited to stronger action by friction, and during the pains gentle traction was made by the feet; but although there was considerable delay in the delivery of the breech, notwithstanding that the pains got better, the expulsion of the abdomen was attended with great difficulty, and eventually was not effected without slight laceration of the perineum. The head and upper extremities came away simultaneously with the body. When the feet were touched, during the time they protruded through the vagina, they retracted quickly; but when the child was born it did not respire. The placenta came away in about fifteen minutes and was apparently healthy.

On examining the fetus, which was of the full time, I observed that it was anencephalous. Its face had that hideous appearance which the French term "*Tête de crapaud*;" the appropriateness of this description was borne out by the midwife, who exclaimed that it was like a toad; its neck and chest were very short, and as it were fused into one piece. The bones of the vault of the cranium were undeveloped, and the integument of the face and neck was continuous with a reddish-coloured structure, composed chiefly of areolar tissue, which immediately covered the rudimentary encephalic ganglia lying on the base of the skull, to which ganglia was connected a perfect medulla oblongata and spinal cord. There were curious malformations of the hands and right foot; the fourth and fifth fingers of the right hand were united by a web up to the distal end of their second phalanges, and there was an additional or sixth finger, with a distinct nail, similarly united to the entire length of the fifth finger. The left hand and right foot had corresponding malformations, viz., an intimate union of their respective third and fourth digits, which appeared as one, but had each separate nails. The left foot was normal.

The abdomen was greatly distended, of a globular form, and dull on percussion; it measured eighteen inches in circumference. On laying open the peritoneum two large tumours were found, one on either side, almost entirely filling the abdominal cavity. The liver was pushed upwards, and the intestines greatly displaced. The stomach could not be then seen. On examination these enormous tumours, to which the size of the abdomen was owing, were found to be the kidneys; and lying on and adherent to their surfaces were the ovaries and Fallopian tubes,

which led to a bifid uterus. A small gelatiniform polypus, attached by a long filamentous peduncle to the labium minus of the left side, hung down between the œdematous lips of the vagina. The bladder was empty. The supra-renal capsules appeared normal. There was no fluid in the bladder. On removing the capsules from the kidneys their surfaces presented the usual lobulated appearance of the fetal organ, but on making a section they were found to be composed of numerous small transparent cysts, visible to the naked eye, of about the size of peas, lying in a matrix of a light greyish colour, from which they could not be detached, and containing a clear serous fluid: the distinction between the cortical and medullary portions was totally obliterated, but the outline of the calyces could be indistinctly traced; the ureters were pervious. In consequence of the wish to have a drawing of the parts taken *in situ*, the kidneys could not be weighed until after their section and exposure to air for some days; even then the weight of the right kidney (both being apparently of the same size) was 6 oz; in length it measured $6\frac{3}{4}$ inches, in width 5 inches, and in thickness 2 inches. The fetus itself weighed 90 ounces; the proportion, therefore, of the weight of the kidneys to that of the entire body would be about 1 to 75, instead of, as in the normal fetus, 1 to 80.

This case, as well as being illustrative of the independent liability of the fetus to disease, is also, if one may judge from the scanty literature on the subject, interesting as an example of a remarkably rare degeneration—interesting alike to pathologists and accoucheurs. Not laying much stress on the anencephalic condition of the fetus, except as regards its connexion with hyper-development in other parts, which I think is unique in this case, I may mention that there are but few examples on record of a similar degeneration. Rayer, in his splendid plates, delineates a case of *atrophy* and cystic degeneration of *one* kidney in a fetus. In this case the cysts were larger, but not so numerous as in mine, and the other kidney was healthy. In Vol. III., p. 514 of his book, he states that examples of this degeneration of the kidneys of newly-born infants are very rare, and he only alludes to three somewhat similar cases. Virchow also, in a paper on “Congenital Dropsy of the Kidneys” (*Verhandl. der Phys. Med. Gesel., in Würzburg*, 1855), relates some instances of cystoid degeneration with complete atresia of the pelvis and papillæ, and refers to this affection as a cause of obstruction to labour. In a case reported by M. Siebold, of Gottingen, in the *Rev. Med. Chir. Janvier*, 1855, the head presented, but the expulsion of the child was arrested by the size of its abdomen. Repeated tractions were required. The circumference of the abdomen was seventeen inches. On inspection two enormous tumours were found in the abdomen; these were the kidneys—the two weighed *two pounds*. Each was six inches long, four wide, and three thick. On removing the capsule,

small cysts, containing a transparent serosity, were seen scattered about. On section the cortical substance could not be distinguished from the pyramids, the greater portion consisting of small cysts visible to the naked eye. In Virchow's and Siebold's cases, and I think in mine also, these cysts were dilatations of the urinary canals.—*December 16, 1865.*

Diphtheria.—DR. FLEMING said that on the 24th December, a young man, having all the outward characters of good bodily health, presented himself at the Richmond Hospital, labouring under what he described as lumps in the throat, accompanied by a sensation of choking. He was immediately seen by the resident pupil, Mr. Bredon, then on duty, and placed in bed, and it was remarked that his respiration was very rapid, that he was in a state of extreme feebleness and debility, and that death was rapidly impending. After a short time he was able to give some little account of the nature of his illness, and made a statement which was afterwards confirmed by his friends. About ten days previous to his application at the Hospital he was attacked with what he called ordinary sore throat. He applied to a dispensary in the neighbourhood where he lived—Parkgate-street, a very healthy neighbourhood—and got some medicine, which he carelessly took. He continued at his work for a week, as porter at the railway adjoining. Shortly after his admission he (Dr. Fleming) happened to be at the Hospital, and was brought to see him, and at that moment he presented all the features that would have indicated in a child croup in an aggravated form. He examined his fauces carefully, and there was an absence of the slightest deviation from a healthy condition, or the slightest fetor of the breath. As well as he could he examined the man's chest; it was impossible to put him with safety into a sitting posture for more than a minute or so; but Mr. Bredon and himself (Dr. Fleming) found that there were râles over the base of both lungs. His pulse was barely traceable, the respiration very rapid, and he was bathed in perspiration. The usual treatment was recommended, and he contemplated having the assistance of some of his colleagues, with a view to any operative interference, when he was suddenly sent for by one of the nurses, who told him the man was dying. He found a very serious change in his symptoms, bad as they were at the time of admission. His countenance presented a ghastly appearance, bathed in perspiration, and with a livid hue over it; the lips were also livid, and the man was pulseless, and gave a sort of jerk in breathing that appeared to be extremely distressing. At this time the neck was not extended, as was generally the case in croup, but was somewhat flexed. He thought at the moment it was advisable to open the larynx, and he did so. The operation was rapidly performed, not a drop of blood was lost, and the tube was introduced without any difficulty.

He was immediately attacked with a sort of convulsive fit of coughing as was usual under such circumstances, and threw up through the tube and mouth a quantity of semi-purulent fluid, slightly tinged with blood. Every two or three minutes he had a convulsive fit, and the fatal termination of the case was evident. Every attempt was made to induce artificial breathing, and the resident pupil, Mr. Bredon, did not hesitate, with that view, to have recourse to a measure sometimes adopted in such cases. Nothing however succeeded, and the poor man died a few minutes after the operation. The history which he gave of his case was confirmed by his friends. He lived with four or five other young men, all of whom were healthy, in the same room; there was no epidemic either of scarlatina or diphtheria in the neighbourhood. Dr. Fleming then exhibited a drawing of the parts as they appeared on a *post mortem* examination, the drawing executed by Mr. Connolly being described as a very accurate delineation of the appearances which were presented. It would be seen, continued Dr. Fleming, that the back of the epiglottis, the inside of the larynx where the vocal cords appeared to be obstructed, and the whole side of the trachea extending into the bronchial tubes, were lined with a membrane tinged with a larger amount of greenish hue than was commonly the case. There was not the slightest firm attachment of any kind between this membrane and the lining membrane of the tubes, but there was a great amount of vascularity existing. The preparation had lost the characteristic appearances marked on the drawing, but yet the membranes were distinctly traceable throughout the whole canal of the trachea and its bifurcations.

As to the nature of this disease there must to a certain extent be some doubt. One might consider it diphtheritic laryngitis or diphtheritic tracheitis. He would not say there were not cases of disease of this kind confined to the larynx and the trachea, but in most of the cases he had seen the fauces engaged, while in the present case nothing of the kind existed. There was no indication of swelling of the neck, the glands were free from enlargement, and the surface of the body exhibited no eruption of any kind indicative of any constitutional affection. Some years ago when he had charge of a medical ward in Steevens' Hospital, he recollected the case of a young man, about twenty years of age, who was in a convalescent state after fever, when he was suddenly attacked with symptoms of croup. Operation was deemed not advisable. He thought Sir Henry Marsh exhibited the specimen at one of the meetings of the society, as illustrative of the condition that existed in this disease. The specimen and drawing much resembled those now exhibited. In the present case the opening of the larynx was effected with ease, and there was no doubt the tube passed within the circle of this morbid exudation. In many instances it had happened in passing the tube, either in tracheotomy or larynotomy, that it passed between the lining membrane of the larynx

or trachea and the abnormal membranes. The case was a most interesting and important one. The opinion of Trousseau as to the advisability of operation in such cases was different from that entertained by surgeons in this country; but it was probable, however, that the cases Trousseau had met with were different from those we were accustomed to here. The lungs presented the appearance usually found in cases of laryngeal or tracheal obstruction.—*January 13, 1866.*

Hypertrophy of the Heart, Cirrhosis of the Liver, &c.—DR. HAYDEN submitted to the society the diseased viscera taken from a man who died within the last few days, of general dropsy. He was aged 44; was a discharged soldier of intemperate habits, and had been several years in India. About four months ago he reported that he observed, for the first time, an unusual frequency in passing water. A month subsequently he noticed a swelling of the ankles, and shortly afterwards of the lower extremities generally, and of the abdomen. He came under Dr. Hayden's notice for the first time on the 8th of January. He was then generally dropsical; the lower extremities and genitals were very much distended, the peritoneal cavity was full of fluid; and the intestines distended with flatus; ramifying over the left side of the abdomen were several large, tortuous, and distended veins, the colour of which contrasted strongly with that of the skin, which was of waxy whiteness. The face was likewise œdematous, as were the arms and chest; respiration was very much embarrassed; the man's pulse was quick, but regular, and rather strong. The heart pulsated with considerable force in the usual situation. Its sounds and its rythm were strictly normal; the precordial dulness was extended. The man was scarcely able to lie down so embarrassed was his breathing. The urine was found on examination to contain albumen in large quantity; its specific gravity was 1010, reaction acid; the precipitate, examined microscopically, exhibited tube-casts of two kinds, viz., some waxy or hyaline, without epithelium, and others of a similar character, but studded with fat granules *vice* epithelium. Respiration was loud on both sides, except posteriorly, and inferiorly on the right side, where it was feeble, and accompanied with râles; here, there was dulness, but elsewhere percussion seemed normal. There could be no doubt whatever that there was disease of the kidneys, and it was equally evident that there was disease of the liver; because the effusion into the peritoneum was out of proportion to the dropsical effusion elsewhere, and further evidence to this effect was afforded by the presence of distended veins on the abdomen, showing visceral obstruction. But still further it was manifest from the great extent of precordial dulness and from the strong action of the heart, that there was hypertrophy of that organ. The idea of valvular disease was negatived by the absence of murmur and irregularity in the heart's action. An

attempt was made to relieve the man by means of an acute purgation with elaterium, but the stomach refused to tolerate the medicine. On the night of the 10th the man vomited up a large quantity of dark discoloured blood. On the following morning he (Dr. Hayden) found him breathing stertorously; pulse irregular and intermittent, and the impulse of the heart was not to be felt. It was manifest that effusion was taking place into all the serous cavities, amongst others the pericardium. The man was quite conscious. He attempted to sit up in bed and to speak, and whilst making this effort died in a state of syncope.

On examining the body after death, an immense accumulation of fluid was found. The liver was cirrhotic; it was lobulated on the surface, and mapped out into small spaces by the thickened capsule of Glisson. The peritoneal investment of the liver was likewise thickened; the organ was contracted in its transverse, and likewise in its vertical diameter; it was increased in diameter antero-posteriorly. On section it did not show an advanced stage of cirrhosis, which accorded with the fact that there had been no olive discolouration of the skin at any time. The spleen was found to be considerably enlarged, and its capsule was thickened and opaque over a space about the size of a crown-piece; this portion of the capsule, which was about one-eighth of an inch thick, projected into the substance of the organ, and by this the spleen was attached to the diaphragm. The splenic artery was dilated, and remarkably tortuous, nearly as large as the arteria innominata; the left kidney was rather larger than this organ was found to be in health. On section it presented rather a mottled appearance. The cortex could not be said to be diminished, it was rather increased in thickness. The Malpighian pyramids were large; a section of the cortical substance examined microscopically showed fatty disease in an advanced stage.

The right kidney resembled the left in general appearance, but was very much smaller; it did not much exceed in size the supra-renal capsule of the fetus.

The heart, as was anticipated, was found to be hypertrophied on the left side. The right auricle and ventricle were rather dilated, as was likewise the right auriculo-ventricular opening; so much so, that four fingers might be made to pass through it with great facility. The left auricle was rather thicker in its walls than in health. The left ventricle was concentrically hypertrophied; the walls being considerably over an inch in thickness in the mid-region, and towards the apex somewhat less. This cavity was contracted; the valves were healthy both on the right and left side. If it be admitted that the left ventricle was contracted in size, the absence of valvular disease was a circumstance of interest. The pleural cavities were full of fluid. The right lung sank in water, the left floated, but he was able to inflate the right with great facility, and then it floated also. This man died certainly of effusion, and not of uremic

intoxication. The features of greatest interest were—first, the hypertrophy of the left cavities of the heart, affording a good example of that condition of the heart which Dr. Kirkes pointed out as frequently associated with Bright's disease; secondly, the small size of the left kidney; and thirdly, the condition of the liver and of the spleen.

The following measurements were taken January, 16th, *i.e.*, five days after the patient's death, when, it is to be presumed, *rigor cordis* had departed, and after much and forcible stretching of the ventricular walls.

Walls of left ventricle measured in thickness, at apex, seven-eighths of an inch; at middle portion, one inch and one-sixth; at base on level of free extremities of mitral valve, one inch and one-third.

Length of cavity of left ventricle from aortic valves to apex, three inches.

Width of cavity of left ventricle from septum in middle of cavity, one inch and a half.

Thickness of left auricle in central portion, one-eighth of an inch.

Thickness of right ventricle at apex, one-sixth of an inch. Thickness in middle portion, one-fourth of an inch. Thickness at base on level of free extremities of tricuspid valve, one-eighth of an inch.

Circumference of entire heart at base of ventricles, eleven inches and three-eighths.

Circumference of left ventricle at base, ten inches and one-eighth.

Circumference of aorta two inches above valves, three inches and one-sixteenth.—*January 13, 1866.*

Gangrene of the Œsophagus.—DR. LYONS brought under the notice of the society a case of gangrene of the œsophagus, arising from a somewhat unusual cause. The man, who was the subject of it, was at breakfast on Sunday week last, and being, as he described it, exceedingly hungry, eating bread and some fried liver, he bolted a large piece of crust of bread which he described as being of the dimensions of his fore finger with a portion of liver along with it. This stuck in his throat, and he made violent efforts by passing his finger in as far as he could, and by repeated regurgitating efforts to dislodge the impacted piece of bread. This he believed he succeeded in doing, and for the remainder of the day he suffered little or no inconvenience; neither did he suffer any inconvenience on the Monday or Tuesday following. On Wednesday he was admitted to Jervis-street Hospital, complaining of a certain amount of uneasiness in the throat. There was a great deal of swelling below the angle of the jaw on the right side, with some tenderness in that situation. He (Dr. Lyons) made a most careful examination of the throat; a burst of strong sun-light which occurred at the moment enabling him to do so satisfactorily. He passed his finger as far down as he could into the throat, felt the upper portion of the larynx very

distinctly, and got his finger a short distance below it, but failed to find anything. Owing to the swelling, and tension, and pain that existed, a few leeches were applied to the throat, and poultices subsequently, and the next day the patient found himself considerably relieved. He seemed to be progressing tolerably well, the swelling was considerably reduced, and he was up, and on last Sunday he (Dr. Lyons) found him sitting at a table in the ward eating a portion of a mutton chop, and he was informed that the man eat one-third of a mutton chop on that day. On Tuesday he requested permission to leave the hospital for a short time, and was out some three or four hours. He returned perfectly sober, and on the next day complained of a considerable degree of uneasiness. Still he was able to swallow fluids, arrow-root, beef tea, wine, and some spirits and water, which he got during the night, on one occasion, when he felt faint. On Tuesday evening a gargle was given him; and in the effort to gargle he brought up a quart of foul, fetid, grumous matter, partly mixed with blood. He (Dr. Lyons) saw him the next day and again carefully examined the throat, but was unable to ascertain the presence of any foreign body. On Wednesday night between six and seven o'clock he took a cup of arrow-root, which he swallowed after some effort, taking it slowly in spoonfuls; and about one o'clock next morning was seized suddenly with an attack of weakness, and threw up a quantity of grumous stuff. Dr. M'Donnell, one of his colleagues, being the accident surgeon on duty, was sent for, and, just as he arrived, the man expired.

A *post mortem* examination was made. The tongue and all the back parts to the spine were removed, and there was found upon the spine evidences of most extensive gangrenous destruction of all the tissues about the œsophagus, extending from the lower part of the pharynx down a long way in the posterior mediastinum, and near to the cardiac orifice of the stomach. He now proceeded to slit open the œsophagus, and found on doing so an obstruction in the shape of a thin piece of bone lying right across the tube. The drawing made by Mr. Connolly, which he now exhibited, showed accurately the position that it occupied. It was from an inch and three-quarters to two inches below the level of the rima glottidis, and it was impossible it could be touched with the finger, or that any physical evidence of its presence could be ascertained by digital or ocular examination. It appeared to him to be a sharp piece of bone, such as would be detached from a rib of bacon. It was curious that the man repeatedly said he was breakfasting on bread and liver, and made no reference to any bony substance, nor had he any suspicion that there was a bone or other substance in the throat, nor did he conceive, after he came into hospital, that there was anything in his throat, and he attributed the pain and uneasiness which he suffered to the violent regurgitating efforts and the violent manipulation which

he had used to get rid of the piece of bread and liver which constituted, in his own opinion, the obstruction in the first instance.

Here, then, was an example of gangrene of the œsophagus, which, as far as he knew, was very unusual from such a cause. The question arose what could have been done supposing they knew that there was a foreign body in the throat? It did not appear to him that operative interference was desirable. He did not see how any mode of procedure could be applied to detach the portion of bone without doing a considerable amount of injury to the soft parts in which it was imbedded; and as to the use of the probang, he congratulated himself on not having used it; for the probang meeting this portion of bone would push it further down, tearing and lacerating the mucous membrane, and causing further destruction of tissue and possibly extensive hæmorrhage, and he should be inclined to blame himself for having caused so much injury and so large an amount of mischief consequent on the destruction of mucous membrane. Bodies impacted in the throat frequently found their way into the stomach, and were then passed *per anum*. Sir Benjamin Brodie spoke with hesitation as to any operative proceeding to extract bodies that could not be felt. He said:—"But let us suppose that the foreign body cannot be felt with the finger, are you then to attempt to take hold of it with the forceps? Really, to extract a foreign body from the œsophagus, below the part at which you can feel it with the finger would be a very difficult operation, and probably not a very safe one; for, in poking with the forceps, you might carry them through the coats of the œsophagus."—Works of Brodie, Vol. III., p. 513.

In this particular instance, where a piece of bone was so very sharp on both sides, it would have created a vast amount of laceration had it been forcibly pushed down. Again, the mere fact that a patient thinks there is a foreign body in his throat is no proof that it is there. Sir Benjamin Brodie, on this point, says:—"There is another matter of considerable practical importance, to which I wish to call your attention, with respect to matters supposed to be stuck in the œsophagus. A woman was brought to town who was thought to have swallowed a piece of bone, and I believe that there was no doubt that she had done so. I introduced my finger, and, not being able to feel it, I concluded that it was below the reach of the finger. I then passed an œsophagus bougie into the stomach, but could not feel it. I then introduced a probang with sponge, but with no better effect; but still the woman had the sensation of its being there. I now began to doubt whether it really stuck there, and to suspect that the sensations that she complained of indicated that some part of the œsophagus had been abraded or torn by the foreign body, but that the body itself had passed into the stomach. It is a common trick with conjurors to put half-a-crown into the hands of a person, to press it firmly, and then to say to him, 'You are sure it

is there?' The party says 'Yes.' In fact he has the feeling of it, but when he opens his hand it is not there."—*Ibid*, p. 517.

It is further worthy of note that the probang has been found, in more than one instance, to pass freely into the œsophagus without detecting a foreign body when somewhat of the dimensions of that in the present case, the instrument passing either in front of or behind the obstruction. Such was the case in an instance which occurred in Jervis-street Hospital, in the practice of a gentleman now deceased.

He thought this case worthy of being brought before the society for several reasons. First of all as an instance of gangrene of the œsophagus arising from an unusual cause. Gangrenous destruction of the œsophagus from swallowing caustic fluids they had examples of; and in the Museum of the Richmond Hospital there were drawings of cases where it had arisen from swallowing sulphuric acid, but he was not aware of any case where a foreign body impacted in the throat had led to such destruction of the soft parts. Again, this case showed that surgeons should not rely too implicitly on the statements of patients as to what they swallowed. It could not well be conceived how a man taking such a breakfast as bread and fried liver could have swallowed a portion of bone, and that without being conscious of it.—*January 13, 1866.*

Cerebro-Spinal Arachnitis, with Paralysis of the Right Third Nerve.—*DR. BANKS* said that on the 10th of October, 1865, a girl, aged twenty, was admitted into the Hardwicke Hospital. She stated that, four days before, she had caught cold, and had two or three attacks of shivering, followed by general uneasiness and pain in her back, and so ill did she feel that she immediately took to bed. On the fourth day she was received into hospital. The symptoms she laboured under were those generally met with in the first four or five days of simple continued fever. Her skin was hot and dry, her pulse 90. She had some slight headache and general uneasiness, and her bowels were constipated.

For three days after admission her symptoms did not undergo any remarkable change. The temperature rose, as ascertained by the thermometer; the pulse rose in frequency, and she had more headache, and her sleep was very disturbed, and there was some slight wandering. On the fourth morning after admission, and the eighth day of her illness, the resident pupil, Mr. Little, who observed the case very closely, ascertained from the nurse that a very extraordinary change had taken place the preceding night. It appeared that her mother had paid her a visit; an altercation arose, which ended in a struggle, and the mother by force removed a little bag of money which was about the girl's neck. After this she became violently delirious, could with difficulty be kept in bed, and raved continually about money. The pulse was more rapid, the temperature had risen, the head was much hotter—in short, there was

marked exacerbation of all the febrile symptoms. For the next three days she was violently delirious, she disliked the light, and her hearing became morbidly acute. She could repeat conversations carried on at the very end of a large ward. On the twelfth day of her illness for the first time a new train of symptoms presented themselves. She was observed, as she lay on her back, to have her head thrown very far back. She could not bear the lightest pressure on the neck, chest, or abdomen. The slightest movement, moving the arm or leg, produced extreme pain, although at the same time she was flinging about her arms and legs in a very violent and excited manner. The delirium continued, the pulse rose in frequency still, as did the temperature, and she spent two or three nights almost sleepless. The character of the delirium resembled that in delirium tremens; it was a busy sort of delirium; she was more engaged about her own sensations than raving about the money which for the first two or three days after the fight with her mother was the only subject of her wanderings. On the fifteenth day of her illness it was observed that there was ptosis of the right eye and great dilation of the pupil, with *divergent* strabismus. She had scarcely any cough up to this time, and the respiration was not more rapid than would be expected from the rapidity of the circulation, but on this day her respiration became extremely embarrassed, and on examination of the chest signs of universal bronchitis were discovered; there was no dulness on percussion. She sank gradually, and for some hours before death she manifested symptoms of death by the chest rather than by the head. The face and extremities became livid, and there were all the marks of malaëration of the blood; and on the 18th she died.

An examination revealed the following morbid appearances:—The cerebral sinuses and the vessels of the pia mater were much distended with dark blood. The upper and lateral surfaces of the brain did not present anything abnormal. The base of the brain was the seat of extensive deposition of lymph of a greenish yellow colour, which had caused it to contract adhesions, particularly to the lesser sphenoidal wing. This deposit commenced in front, a few lines beyond the optic commissure, enveloping it completely, also covering and concealing the roots of the first nerves and closing up the entrance of the fissure of Sylvius. From thence proceeding backwards it completely covered and concealed all the parts situated in the interpeduncular space, and enveloped, for a considerable distance, both crura cerebri. Over the pons varolii it was very thick, diminishing towards its posterior edge, and the arachnoid covering the medulla oblongata was merely thick and opaque. The deposit was more abundant, and the layer was thicker, on the right than on the left side, especially where it covered the crus cerebri and the pons.

The third pair of nerves, in common with the other parts contained in

the interpeduncular space, were enveloped in lymph; but on examination it was found that the right nerve had undergone a remarkable diminution in size, when compared with its fellow on the other side. The right nerve appeared to have scarcely half the diameter of the left.

On opening the theca of the spinal cord the arachnoid was found thickened and opaque, with deposition of lymph over its entire extent. The amount of lymph varied in different situations; it was very slight on the medulla oblongata and upper cervical part, was thickest at the brachial enlargement and in the spinal region, but was distinct down to the very middle of the cauda equinæ. The lymph on the spinal arachnoid presented the same yellowish-green hue as at the base of the brain.

In the lower and anterior part of the lumbar enlargement a small superficial effusion was found in the meshes of the pia mater.

The *lungs* were the seat of miliary tubercle, sparsely scattered everywhere except at the posterior parts, where they were rather closely set; the bronchial membrane was inflamed.

The *spleen* was large, and studded with tubercles, which were much more abundant than in the lungs.

The mucous membrane of the small intestines was the seat of inflammation, particularly the jejunum and the ilium, towards its termination; and the patches of Peyer were more distinct than in health; but there was not a trace of ulceration.

Dr. Banks observed that the pathological appearances in this case, so far as the brain and spinal marrow were concerned, resembled those which were observed in 1846, when cerebro-spinal arachnitis appeared in an epidemic form. Some of the symptoms were identical with those of that terrible disease, but in other respects it differed in a remarkable manner.

He particularly called attention to the duration of the disease, which contrasted with the fearful rapidity which was one of the most striking characteristics of the epidemic disease.—*January 20, 1866.*

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.^a
 TWENTY-EIGHTH ANNUAL SESSION.

DR. M'CLINTOCK, President.

Remarks on Gynecomazia. By Dr. FOOT.

Under the title of gynecomazia I wish to make a few remarks on the development of the mammary gland in the male. The term gynecomazia is not the coinage of modern days, it is a classical word employed in the second century of the Christian era by Galen, in his four hundred and third definition, to signify an unnatural development of the fat underlying the breast, and is also alluded to by Paulus Egineta (Lib. VI., Sect. 46). The first time any attention was drawn to enlargement of the breast in the male subject was in 1863, when I saw a boy, fourteen years of age, with the mammary gland on the left side strikingly developed; the gland on the right side was of the usual size. There were no other appearances of effeminacy about him: he was stout and manly, well grown for his age, his voice was becoming deep, and the sexual hair appearing on his chin and cheeks. The breast had a firm glandular feel, hung downwards from its weight, was conical in shape, the nipple surrounded by a pale pink areola, studded with papillary projections. There had been no secretion observed from it. The development of the breast was discovered during the examination of his chest for other purposes: it gave him no pain or concern, and he was surprised at my wishing to take a cast of it. No attempt was made to reduce its size, as there was no indication for any interference with it. The next case I met with was in a man, between twenty and thirty years of age, when both breasts were equally developed, and had the external appearances of the virgin mammary gland. He said a thin white watery liquid sometimes came from one nipple. I never saw this occur with him, and I tried in vain with a strong breast-pump to get any fluid from the lactiferous ducts. He was not unusually muscular, but the facial hair was present in abundance, and there was nothing feminine about him. In the next case there was a cystic tumour lying superficial to an enlarged mammary gland. It occurred in a lad of sixteen, of manly manner and appearance. He suffered pain from the cystic tumour, which had existed a year at the time I saw him, but had lately been getting larger, and becoming painful. The left breast was the one affected. The tumour was seated at the base of the nipple, was about the size of a large gooseberry, tense but fluctuating; no fluid had been discharged from it. His friends were alarmed at the pain and swelling of his breast, and made him get advice about it, much against

^a These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

his own inclination, as he seemed quite ashamed of having, what he thought, such a feminine attribute about him. The ointment of iodide of potassium applied locally, with protection of the breast from his shirt front by a layer of cotton wool—and the iodide of potassium internally, reduced the size of tumour, and lessened the pain; then tight strapping with adhesive plaster almost restored the gland to the size of the opposite one. There are reasons for believing that the use of the compounds of iodine is particularly applicable in such cases. There is nothing in the anatomy of the male mammary gland to forbid its assuming the form and functions of the female breast, though its parenchyma is scarce, appearing as a white fibrous mass, without showing any arrangement into lobules, and the glandular tissue displays only few of the vesicles so numerous in the female mamma; yet, both in man and in the lower animals, there are many instances in which the male breast has become enlarged and secreted milk most actively. It is well known to the members of this society that this tendency on the part of the male breast is especially marked in the infant a few days after birth, and again at the advent of puberty. Microscopic examination of a specimen of milk from the mammæ of a male child, nine days after birth, has shown the presence of numerous milk globules; and chemical analysis has confirmed the testimony of the microscope. In the museum of the late Dr. Montgomery was the breast of a man who had mammæ like a young woman. Bristles are passed through ducts in the nipple into the substance of the gland; the genital organs were well formed. He was forty-five years of age, had never married, and died of phthisis.—*Signs and Symptoms of Pregnancy*, p. 125.

In the conclusion of his third book of the *History of Animals*, Aristotle speaks of a male goat at Semnos which yielded milk enough for cheeses to be made from it, and whose male descendants inherited the peculiarity; and Isidore Geoffroy St. Hilaire had for some years, in the Jardin des Plantes, a male goat with largely developed udders yielding milk (*Aristotle*, G. H. Lewes, p. 285). It is remarkable that the male mammary glands are seldom both enlarged, it is generally on the left side the anomaly exists. John Hunter gives brief notes of five cases of enlargement of one breast in men, from sixteen to twenty-seven years of age, and then cites the case of a man who nursed eight of his children from his left breast (*Essays and Observations*, edited by Prof. Owen, Vol. I., p. 238). In 1855 Dr. Tufnell (*Dub. Quart. Jour.*, Vol. XIX., p. 230) read before this society the particulars of the case of a soldier of the 60th Rifles, whose left breast was developed in form and size equal to that of a female at puberty—the nipple was prominent, the areola well marked, of a pink colour, and much brighter than that of the opposite side. The man's attention was first drawn to the enlargement of his breast when about twelve years of age. His sexual characteristics were fully marked.

There are many other recorded instances of this unilateral development. In the case of an athletic blacksmith whose left breast became, without cause, in six weeks, as large as that of a nursing female, and was filled with the lacteal secretion, soap plaster and compression reduced the gland to its natural size.—*Brit. For. Med. Ch. Rev.*, Oct., 1850, p. 551.

Pliny alludes to the African race called Androgynæ, whom the ancients reported to have a female breast on the left side, and a male one on the right.—*C. Plinii Hist. Nat.*, Lib. VII., Cap. 2.

The development of the mammary gland in the male may or may not be attended with an arrest of development of the genital organs. The testes are often observed to be small and soft when the male breasts are enlarged. In such cases it appears as if the adoption of one sexual character which does not properly belong to the male is compensated by the loss of another which does. The same thing is observed frequently in birds—hen pheasants, for instance, may partially adopt the voice and plumage of the male; and it has been pointed out that those hen-birds who do so are always affected with disease of the ovarium, and that those in which disease has made most progress have advanced furthest towards the male in external appearance (*Proc. Zool. Soc.*, Lond., 1860, Part 3, p. 373). In cases of disease of the ovary in domestic hens, the plumage remains perfectly unchanged, but the comb and wattles become extraordinarily developed, in many cases even surpassing those of the male bird in size. The birds crow like the males, and are popularly known as hen-cocks. The alteration has been observed as resulting from melanosis of the ovary, from cartilaginous degeneration, and from a generally diffused inflammation arising from the escape of an ovum from the oviduct.—*Op. cit.*, 1857, Part 25, p. 81.

Paulus Egineta, speaking of male breasts resembling the female, says, as at the season of puberty the breasts of females swell up, so in like manner those of the males also swell to a certain extent, but for the most part they subside again. In some cases, however, having acquired a beginning, they go on increasing, owing to the formation of fat below. Wherefore, as this deformity has the reproach of effeminacy, it is proper to operate upon it. He then proceeds to describe the operation for its removal.—*Paulus Egineta*, Vol. II., p. 334, *Syd. Soc. Trans.*

It is now known that the enlargement is not merely due to formation of fat lying between the gland and the pectoral muscle, but to a true development of an organ which was intended to remain in a rudimentary condition.—*February 10th*, 1866.

DR. TELFORD exhibited a specimen, and read the notes of a Case of large ovarian tumour of three years' standing; subsequent ascites, paracentesis; death twelve hours after; autopsy.

Mary Howard, aged thirty-seven, married, mother of five children,

three years since perceived an enlargement in the left side of the abdomen. She did not, however, pay much attention to it till ten months after its appearance, when she was inconvenienced by its increasing size. She then sought advice at the Rotundo Hospital, when the enlargement was found to be due to an ovarian tumour originating on the left side. In other respects her health was very good, the menstrual function being quite regular. The tumour was quite movable, and in every way she seemed a very fit subject for ovariectomy, which was proposed, but owing to the advice of her friends, she refused to submit. She then left hospital, and shortly after conceived, in April, 1864, and was safely delivered, at the full time, of a healthy child on the 15th of January, 1865. Her convalescence was natural. The tumour now rapidly increased in size, causing her great inconvenience, and a good deal of dyspnea. She was readmitted to hospital on the 17th of January, 1866, just one year after her confinement, when she presented the following symptoms:—General appearance cachectic; pulse rapid and weak; breathing laboured and difficult. On examining the abdomen it was found to be enormously distended by a large tumour, which in some parts feels very firm and resisting, the remaining portion conveying a very distinct sense of fluctuation. On placing the hand over the abdomen, a peculiar crepitating crackling feel is conveyed, such as is found in cases of emphysema. The hand passes easily between the pubes and the tumour. On placing the hands on either side of the tumour it can be lifted up, and appears quite movable, gravitating to whatever side the patient lies on. It is distinctly multilocular, several prominent cysts being felt through the parietes; the uterus is prolapsed. She was treated by nutritious food, wine, and preparations of iron. The prolapsed uterus was replaced, and kept in position by a large pessary. Owing to the enormous size of the tumour, the cachectic condition of the patient, and the presence of some ascites, an operation was not deemed advisable. She then returned home. On Wednesday, the 7th of March, she requested to be tapped, the ascites having greatly increased since her dismissal. We found her very weak and feeble, with very difficult breathing—in fact, I may say dying. However, as she thought the tapping might give her some relief, the operation was performed in the mesial line, midway between the pubes and umbilicus, when about twenty pints of a brownish fluid something resembling muddy beer, were removed. She gradually sank, and died twelve hours after the operation.

Autopsy.—Permission was obtained to examine the abdomen only. The surface of the body was not as much emaciated as one would expect from such a severe illness. On opening the abdomen, a large tumour protruded through the line of incision. A few small adhesions existed on the anterior of the tumour, which gave way readily; the superior and inferior portions seemed totally free, but the posterior was firmly adherent

to the omentum, several large vessels ramifying from one to the other. The abdominal surface of the peritoneum was studded over with a number of tuberculous-looking masses, and the liver was adherent to the peritoneum. The tumour was now removed; but unfortunately, from the small incision we were allowed to make, the pedicle was broken down in separating the adhesions. The right ovary was also removed, and though not much increased in size, found to contain a number of small cysts. On examining the tumour it was found to be multilocular, containing eighteen distinct cysts, some of them contained within the other; it weighed twenty pounds, and was thirty-five inches in its greatest circumference. On opening some of the cysts, they were found to contain a milky-looking fluid, of the consistence of thin starch. Different parts of the tumour, and its fluid contents, were examined by the microscope, but no evidence of malignant disease was discovered.

This case, I think, very well illustrates the difficulty the surgeon has to contend with when consulted as to the advisableness of an operation in cases of ovarian disease, for here, though the tumour was believed to be unadherent, the *post mortem* showed most extensive adhesions. Some of them may, perhaps, have been recent, but others were certainly of some standing, judging from the size of the blood-vessels which passed from one to the other. Another interesting point in this case is the fact of the patient becoming pregnant, and going to the full time, while she laboured under such a severe form of disease. The fact of a patient becoming pregnant may occur often enough in this disease, but abortion is the usual result.—*March 20th, 1866.*

A Specimen of Disease of the Ovary. Exhibited by Dr. DENHAM for Dr. Babbington, of Derry.

Maria S., aged forty-five, unmarried, had one child, more than twenty years since, admitted into Derry County Infirmary 26th of September, 1865. States she had enjoyed good health till about three weeks since, when she began to complain of her stomach being out of order and swelled occasionally. Has menstruated regularly till five weeks since.

On her admission she complained principally of gastric irritation. On examination, there was fulness, enlargement, and tenderness on pressure, in the region of the right ovary. Leeches were applied; a blister; iodide of mercury ointment; tincture of iodine, bi-chloride of mercury administered internally, also iodide of potash and bromide of potash. All treatment proved ineffectual; the swelling and fulness increased.

About second week of October the disease appeared to have extended to left side, and there were indistinct evidences of some slight fluctuation. She menstruated in the second week of October.

The swelling and distention of the abdomen increased rapidly, accompanied with constipation, vomiting, and considerable general prostration.

No very acute pain was complained of at any period of her illness, and even to the last a hopeful looking forward to recovery. The distention rapidly increased, and distinct fluctuation being evident in the right ovary. It was tapped on Wednesday, 15th November, and 110 ounces of thick tenacious yellow-brown fluid drawn off, but with difficulty, as a long silver catheter was passed through the canula, and evidently broke down several cysts.

This operation did afford temporary relief, but she sank, and died exhausted, without pain, on Monday, 20th November.

In presenting these morbid preparations to the society, through my friend Dr. Denham, I have no observations to make. The *post mortem* examination revealed a frightful amount of diseased structures. In the right ovary, in which the ovarian disease was first detected, and from which 110 ounces were removed, the disease appears to be the form described and delineated by Professor Simpson in his lecture in the *Medical Times and Gazette*, October 29th, 1859; while the ovarian tumour of the left side is produced, in same page, as a section of a colloid tumour of the ovary. There is some colloid disease, also, in right ovary. The liver was full of diseased deposits, and the internal surface of the peritoneum was studded with a granules of a similar nature.—*March 20th, 1866.*

Inflammation of the Bladder and Vagina. By Dr. ATTHILL.

There are no forms of disease productive of greater distress and suffering than those affections of the bladder which are met with not only in married but unmarried females; and yet, while far from being uncommon, they seem to have attracted but comparatively little notice. Dr. Churchill has an excellent chapter on "Reflex Irritation of the Bladder" in the last edition of his work on disease of woman, and the late Professor Montgomery read an interesting paper a few years ago on "The Sympathy between the Uterus and Bladder," before the Medical Society of the College of Physicians; but I do not remember having seen the subject noticed elsewhere. I therefore thought the following remarks, founded on cases recently under my observation, would not be deemed uninteresting.

In considering these affections, it is necessary to divide them into three classes, which, while presenting great similarity in their general symptoms, depend on very different causes, and require for their cure very different treatment. We have, then,

1st. Those in which inflammation of the mucous membrane of the bladder occurs as a primary disease, either of an acute or chronic character.

2nd. Those in which inflammation, always in a chronic form, exists

in the bladder, this inflammation being generally a secondary affection, depending on some unhealthy action in the vagina or uterus, but occasionally being a primary affection, the anomalous action in the uterus or vagina being secondary to it.

3rd. Those affections of a purely reflex nature, in which, while all the symptoms are referred to the bladder, that organ is perfectly healthy, they being entirely due to, and caused by, some irritation, morbid sensibility, or inflammatory action, in uterus or its appendage.

The first of these forms does not fall particularly within the province of the obstetric practitioner. Every physician and surgeon is familiar with its character and symptoms. I shall not, therefore, dwell on the subject, but merely mention a few particulars of one case, which was mainly remarkable from the large quantity of pus secreted by the bladder, with comparatively slight constitutional disturbance.

An unmarried lady, aged about forty-five, was seized with a rigor, after exposure to cold. When I saw her some hours subsequently, she presented the symptoms of an ordinary febrile attack. She also complained of very acute pain in the back, which distressed her greatly. On the following day there was no improvement; on the contrary, the pain in the back was worse, and there was also a sense of soreness above the pubes, which was increased by pressure. In addition to general treatment, linseed poultices and other local applications were had recourse to, but with very partial benefit. On the next day she was, if anything, worse, constant desire to micturate being now a prominent and distressing symptom. I shall not, however, weary you with details of ordinary treatment; it is sufficient to say that on the eighth day of her illness I observed pus in the urine, and that it continued to be secreted in large quantities for a considerable time. The exact quantity which was daily voided could not be accurately ascertained, as it was mixed with mucous and other deposits, but certainly it could not have been less than three ounces daily. Nevertheless the patient improved; the pus gradually diminished in quantity, she was convalescent in about a month, and has since that time, now more than a year ago, enjoyed excellent health.

Cases coming under the second head are by no means unfrequent. In them we have well-marked symptoms of irritation, if not of inflammation, of the bladder co-existing with some unhealthy state of the uterus or vagina, and sometimes of the ovaries also. The existence or non-existence of actual inflammation of the bladder being that which distinguishes these, from those in which the symptoms are due to mere sympathy or reflex irritation, the cases we now alluded to form a most important class, the treatment of which require special care, for we are apt to fall into error if, having detected disease in one set of organs sufficient, probably, to account for all the symptoms, we neglect to investigate the condition of the others, and it by no means always

happens that the cure of the one affection, even though it should have been the primary, will be followed by that of the other, unless special treatment be adopted for its relief. The following case will, I think, illustrate this view :—

About a year ago I was consulted by letter relative to the case of a young lady residing in the country. The statement I received was to the effect “that the patient had been an invalid for nearly eight years. Her illness first began with a feeling of weight and distress in the lower part of the abdomen. This was so great as to prevent her walking, and was accompanied by pain all over the lower portion of the abdomen, and in the back. There was also irritation and pain about the vulva, especially during micturition, which always caused considerable distress. After she had suffered in this way for about three months, she was attacked with acute pain in right side of the abdomen. This was followed by shivering fits, which came on at intervals of about a fortnight. After a time a tumour was detected in the right side. The rigors and attacks of pain continued, however, to recur at nearly regular intervals for about a year, when they ceased, and from that time her condition began gradually to improve. For the last six years she has been perfectly free from these distressing symptoms, and of late fancied that the tumour had rather decreased in size; her general health, also, became pretty good; but the pain and soreness across the abdomen, immediately above the pubes, still remained. This prevented her walking, or indeed making any exertion. She also constantly suffered from considerable uneasiness in her side; but that which caused her the greatest distress, and indeed made life a burden, was the incessant desire to micturate, she being seldom able to remain more than an hour, never more than an hour and a-half, without passing water, and this even when in bed.”

As I declined to give any opinion, unless I had an opportunity of seeing and examining the case, the lady came to town, and placed herself under my care. I should mention that I also received a communication from her medical attendant, a gentleman who is in considerable practice in the country. He stated that “the tumour in Miss — side puzzled him very much. To his knowledge it had existed for upwards of seven years. At one time he thought it had changed its position, having been low down in the inguinal region, whereas it now lay nearly under the ribs, he had never been able to detect any attachment to the other organs, and candidly admitted that he was unable to give an opinion as to its nature; but that by far the most distressing symptom in the case was the extraordinary irritation of the bladder and vagina from which this young lady suffered, and which of late had become much more severe—so much so that life was a burden to her. He had tried all kind of treatment, tonics, sedatives, alkalines, and acids, without benefit. Sometimes he thought that a tumour or calculus might exist in the

bladder; but as the symptoms were vague, and the patient a diffident unmarried woman, he had not considered himself justified in making a special examination." I found Miss — to be a very thin delicate-looking young lady, aged about twenty-six or twenty-seven years. Her colour was good, nor was there any unhealthy look about her. I did not obtain much additional information from her replies to my questions, except that she had from time to time been under the care of several medical men (among the rest that of the late Mr. Rynd, who had blistered her side), without obtaining any relief. The tumour referred to was very easily felt immediately under the ribs, the upper extremities being quite close to the liver; indeed, I think when lying on her back it must have been in contact with that organ. It was about six inches in length, and probably nearly four inches in circumference. It was completely above the pelvis, and certainly was not ovarian.

Handling it did not cause much pain, though she stated that occasionally it was very tender to the touch. She also informed me that at the very commencement of her illness, and before the tumour was detected, she was conscious of a sensation as if something had given way in her side. The tumour was, however, evidently totally unconnected with the affection of the bladder. It was not in contact with it, and pressure, even though made forcibly downwards, did not cause any uneasiness in that organ; therefore, though its size and weight must have caused considerable distress, her sufferings could not arise from that cause.

On proceeding to examine the condition of the uterus, vagina, and bladder, I found the former to be normal in size, and apparently healthy, but the vagina was very tender to the touch, and its mucous membrane greatly congested. I introduced a silver catheter into the bladder, but it did not contain either calculus or tumour. The urine was pale, of low specific gravity, and contained a considerable deposit, consisting mainly of mucous, with a small quantity of pus mixed through it. The analysis of the urine was made by my friend Dr. Head. The presence of pus in the urine, taken into consideration with the whole history of the case, led me to conclude that this lady was suffering from chronic inflammation of the mucous membrane of the bladder; that the inflammation of the bladder was probably the primary disease, that of the vagina the secondary; that the attack, originally acute, having been overlooked or neglected, had assumed a chronic form, and now gave rise to the distress this patient suffered. Acting on this supposition, I proceeded to treat the case by injecting the bladder with a solution containing ten grains of nitrate of silver dissolved in two ounces distilled water, to which was added, at the moment of use, twenty drops of opium wine. This treatment was steadily persevered in, the bladder being injected every third day. When first employed this gave considerable pain, but after a little time ceased to cause any distress, and I was now able to increase the

strength of the solution, and used ten grains to the ounce of water. The result of this treatment was very striking—in less than a fortnight she was able to retain her urine for fully two hours and a-half during the day, and for more than three hours at the night. Before another fortnight elapsed she was still farther improved, and now was seldom disturbed more than once during the night, and in the daytime easily passed three hours without inconvenience or distress. But at this point improvement ceased, and I was disappointed at finding that the inflammation of the vagina had but slightly diminished; as this produced considerable distress, I, though with reluctance (the patient being unmarried), made use of the speculum, and applied a twenty-grain solution of nitrate of silver to the whole surface of the vagina, every portion of which was intensely congested. This treatment I repeated twice a week for some time, at the same time persevering with the injection to the bladder. At the end of two months from the time that I first saw her she returned home, her condition being in all respects greatly improved. She was much stronger, had gained strength, and, though still unable to walk any distance without distress, could spend several hours daily in the open air. She could, without inconvenience, retain urine for more than three hours, and was never disturbed more than once at night, and that generally at about 5 o'clock, a.m. I have heard from her repeatedly since. She continued to enjoy what she describes as “great comfort;” and in one letter says, “I hardly know myself so great is the relief I have experienced.”

The treatment employed in this case was suggested to me by the late Dr. Hutton. He was treating in this way a lady who was suffering from a severe form of inflammation of the bladder, when ill-health compelled him to go abroad, and he advised me to continue it with her for some time longer, the patient having placed herself under my care. On that occasion he told me that it was the only treatment he had found of any benefit in such cases, but that it was necessary to continue it for a considerable time, and in severe cases for not less than three months; and the results which have followed its use in my hands fully confirm the truth of this opinion.

In the foregoing there was no doubt but that inflammation of the mucous membrane of the bladder existed; but in by far the greater number of instances which come under the observation of the obstetric practitioner such is not the case. The patient refers her symptoms to the bladder, but on investigation that organ proves to be perfectly healthy. Dr. Churchill is the only writer who has called special attention to this subject. Dr. Montgomery, in the paper already referred to, narrates the particulars of cases in which a marked sympathy existed between the uterus and bladder, but he seems to have overlooked the most important, because the most common, of all the causes which give rise to this most distressing affection which is termed by Dr.

Churchill "reflex irritation of the bladder"—I mean, inflammation of the vagina itself. We have all seen instances in which cancer of the uterus, or even simple ulceration of the cervix, gives rise to irritation of the bladder, but after all they are (at least so far as my own observations go) rare in comparison with the cases in which vaginitis, simple and uncomplicated, is the sole cause. The existence of vaginitis itself is often overlooked, and instances have occurred to me in which the patient was assured that there was nothing wrong with her, because the os uteri alone was looked at, and the vagina itself, though intensely inflamed, entirely forgotten. I do not now allude to acute inflammation of the vagina, which occasionally follows parturition, nor to the acute attacks which sometimes come on after injuries or excessive sexual intercourse, but to the chronic varieties, which are met with frequently in practice. They are, I think, more commonly seen in the weakly and delicate than in the robust female. Frequent desire to make water, and, in severe cases, constant straining to do so, pruritus, sometimes smarting under micturition, and, not unfrequently, pain above the pubes, are the most common symptoms. In married women the act of coition often gives pain, and when this is the case the inflammation will be found to have extended almost to the vulva. This last symptom will aid in distinguishing vaginitis from ulceration of the os or cervix uteri, in which it is seldom observed, especially if it be of a cancerous nature. I lately saw a patient in whom nearly the entire of the cervix had been destroyed by cancerous ulceration, yet she did not suffer from irritation of the bladder, and sexual intercourse was painless. An examination of the vagina with the speculum, in patients suffering from vaginitis, generally gives pain; but a speculum must be used, for it is essential not only to see the condition of the mucous membrane of the vagina, but to apply our remedies through it. There is no occasion, however, to use a large instrument; a small speculum will answer equally well, and give much less distress to the patient. The vagina, on examination, will be found to present either a uniform deep red tint, or else to be covered with white aphthous looking patches, or even sometimes with a herpetic eruption. In either case a cure of the local disease will be generally affected by the application of a solution of nitrate of silver, joined to the use of a cold or tepid douche twice a day, and a tonic treatment; purgatives, except in cases of plethoric women, are to be avoided. Some patients will bear the application of a thirty or forty grain solution of nitrate of silver without its causing pain, but sometimes even a weak solution produces much suffering, and in one instance, that of a delicate married woman, I was obliged to discontinue its use altogether. This fact suggested to my mind the idea of applying, in similar instances, the caustic solution in the shape of spray, by means of one of "Maunder's spray producers," and though I have only used it in a couple of cases, I think it well worth a trial. You can apply the

spray through a speculum equally and evenly to all parts of the vagina, without irritating the inflamed surface by the contact of a rough brush or piece of lint. Rest in the horizontal position, if you can get the patient to carry it out, is, in severe cases, of great benefit. As the inflammation of the vagina subsides, the irritation of the bladder diminishes; and, if it depend solely on that cause, will probably entirely disappear. Vaginitis, though more frequent in married women, is not unfrequently met with in the single. I have seen it in three patients who were undoubtedly virgins, one of whom, a strong, healthy looking country girl, suffered so intensely from irritation of the bladder that life was a burden to her; yet the bladder, in her case, was perfectly healthy. In addition to vaginitis, reflex irritation of the bladder may be caused by inflammation, erosion, or ulceration of the cervix uteri, or by that condition of the canal of the cervix which gives rise to uterine leucorrhœa. There is also one other affection which I have noticed as causing it—I mean simple hypertrophy of the uterus. This, which is not very common, seldom benefits much from treatment. It is, I think, generally met with in women who have reached middle life, and are otherwise healthy. It appears to me to act merely mechanically. The enlarged uterus pressing against the neck of the bladder irritates it, as it sometimes does in the very earliest stage of pregnancy. I recently saw an instance of this in a lady who has not long returned from India, where she had resided for several years. She had never been pregnant. The uterus, otherwise healthy, was much enlarged, and she suffered considerably from irritation of the bladder, especially during the early part of the day. I have also seen very similar cases in other patients who have resided in India, and I cannot help thinking that the habit of taking riding exercise, which is very common there, and sometimes even, as this lady acknowledged, during the menstrual period, must, by determining an undue amount of blood to the uterus, predispose to hypertrophy of that organ.—14th August, 1866.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.*

SESSION 1864-65.

PRESIDENT in the Chair.

On the Treatment of Pneumonia. By PROFESSOR O'CONNOR, M.D.

Within the last month Dr. Hughes Bennett has circulated, amongst the physicians in attendance on hospitals within these kingdoms, a

* These reports are supplied by Dr. Curtis, Secretary to the Society.

pamphlet having for its object to induce them to unite in determining, by statistical returns, the treatment most successful in the cure of pneumonia. He furnishes in this pamphlet forms which he wishes to be adopted in making the reports, and hopes that the result may be a final settlement of this disputed question by the British Association, to which these returns are to be submitted at its next meeting. Nobody can doubt the benevolence of his purpose; and if there was a hope that the scandal arising from warm discussions amongst physicians could be removed in this manner we should hail it as a great blessing. Unfortunately for me I am not sanguine about the result, but still I will humbly co-operate with the enlightened and benevolent purpose of its originator. When all these returns shall be collected they will have been furnished by men of different capacities for observation, different amounts of industry, prejudices produced by temperament, association, and habits; the cases themselves differing in circumstances of age, constitution, sex, previous habits, locality, type of disease, and with different complications, accidental or consecutive. They will then very much resemble the leaves in the Sybilline cave, which would require a sybil to reduce them to intelligible order. Dr. Bennett's treatment is what he calls the restorative plan, in contradistinction to the stimulant on the one side and the antiphlogistic on the other—the Scylla and Charybdis of medical therapeutics. If your patient is to escape being wrecked on brandy or sunk in the whirlpool of starvation and bleeding, he must seek the middle course of eight ounces of port wine, with beef-steak and chops. The treatment, in Dr. Bennett's own words, consists of salines, small doses of acetate of ammonia, with $\frac{1}{4}$ -grain of tartar emetic, diuretics, spirit ætheris nitros, and trœ cholchici. Nutriment—beef-tea and milk, taken early, with beef-steak, mutton chops, and eggs as soon as they could be eaten by the patient. The words are, as soon as they could be eaten by the patient; I think it would be better to have said as soon as they could be digested. People in the humbler classes of life would eat meat at the peril of their lives—as one of the good things they could never easily obtain. A physician is in charge of his patient in every respect, and is bound to supply the place of his lost judgment, and, perhaps, depraved instinct. I confess, as a rule, I would much like to see the pneumonia out of doors before I saw the beef-steak come in; which would require very pungent sauce to make it palatable where there is a loaded tongue, quick breathing, and a fevered brain. Dr. Bennett proves the propriety of his treatment, first, by its success as compared with every other system of treatment; his mortality being *nil* in 125 cases, whereas the treatment by bleeding ranged from $1\cdot7\frac{1}{2}$ to $1\cdot28$; by tartar emetic from $1\cdot4\frac{1}{2}$ to $1\cdot5\frac{1}{2}$; by diet alone from $1\cdot7\frac{1}{2}$ to $1\cdot15$; by stimulants, as by Dr. Todd, $1\cdot9$. And next he enters into an investigation of the pathological changes which take place in pneumonia, from which he deduces, as a necessary consequence, that his treatment is the

only rational one. Taking first his statistical returns one would be inclined to receive them as conclusive evidence of the superiority of his treatment, if we were not aware how fallacious statistics are frequently found to be. We know that certain men go into the great gambling houses, and with nothing to rely on but a run of luck break the bank as it is called; and it is possible that chance may affect the character of a man's patients, and therefore, indirectly, the results of his treatment. In addition, though I must admire the candour and the true professional zeal of Dr. Bennett, I cannot but doubt that he should include in his returns four fatal cases of pneumonic complications to which he refers, and which would make his cases 1·30; and that some weight should be attached to thirteen other cases which he says were found after death to have had pneumonia, which he, however, says was only secondary. The difficulty is to know whether other physicians would not have included in their returns cases of a similar character. I do not believe Dr. Bennett capable of straining a point to advance his favourite theory, but I cannot say the same of clinical clerks, who, as a class, are very well disposed to back up their master's views—*jurare in verba magistri*. I knew it to be the case when I was a student, and I suppose it occurs still. However the challenge thrown down to the profession is a fair one on the part of the distinguished professor, and would lead to beneficial results if medical statistics were not attended with difficulties such as I have already indicated.

I shall now briefly refer to the deductions he makes from pathology in support of his treatment. He states first, in contradiction to many other pathologists, that before the lymph, which is the product of pneumonia, can be absorbed into the blood it must first be converted into pus globules, which again are to be broken up, and disintegrated, and taken into the blood, and then to be eliminated by the kidneys or other emunctories. That these pus cells, to use his own words, must be regarded as living growths, and as such require an excess of blood, good nutrition, and exalted vital force to hurry on their development, and carry them successfully through the several stages of their growth. Now I shall not at all enter into the controversy amongst great microscopic observers, as to whether suppuration is an essential part of inflammation in every circumstance where exudation takes place, I shall merely remark that if it be, it is very different from what practical physicians call suppuration—that which causes the breaking down of tissue, and is productive of great constitutional as well as local changes, so different in kind and degree that giving them the same name is calculated to lead to great and serious errors in treatment. It is little matter to us how the process is conducted if we can prognosticate that the exudate is re-entering the blood without any destruction of tissue, and without constitutional disturbance, and that it is a salutary healthy change. The fact asserted

is, however, of little importance compared with the inference deduced from it—namely, that because pus cells are a living growth they require for their development excess of blood, &c. In order to see the correctness of the inference we must understand clearly all the terms of the proposition. Let any one answer me as to what is meant by an excess of blood? Would not this imply that there was some ascertainable quantity of blood which is to be taken as a standard, and above which standard the quantity of blood in one attacked with inflammation should be kept. We all believe that the quantity of blood in persons enjoying comparative health may be above or below what is most conducive to the perfect performance of their functions by all the organs of the body; and therefore we use the terms *plethora* and *anemia* to indicate the departure from this standard in opposite directions; from which it follows that an excess of blood may be detrimental to the performance of every vital function. Take as an instance the distressing state of many females in suppressed menstruation, unless where that suppression arises from pregnancy, when the increased quantity of blood finds a new channel without overloading the vessels of the mother; and when parturition has taken place the tendency to the formation of more blood than is necessary for the individual finds a vent at the *mammæ*. How often is headache relieved by epistaxis both in sickness and in ordinary health; and who is not acquainted with the fact that the drying up of habitual sores, or of eruptive diseases of the skin, or the cessation of a diarrhea in teething, will produce most dangerous pressure on some internal organ? We come then to the fact, that before the attack of pneumonia our patient might as well have had too much as too little blood to carry on the healthy functions of the body, and that so far as this circumstance is kept in view one patient may require stimulants where the other may require depletion. Now what is our experience? that whereas exudation, that is disease, takes place with a quick exalted action of the heart, absorption, which is recovery, takes place best when the normal condition of the pulse has returned. The continuance of the full quick pulse we look on as a continuance of the disease, even when our investigations do not enable us otherwise to discover its existence. There are many other vital functions going forward in the body at all times as well as the conversion of lymph into pus cells, and we do not consider it necessary to give stimulants to forward them. All the functions of life are never in so healthy a condition as immediately after recovery from a typhus fever, although the debility is then excessive, and the quantity of blood is at a minimum. The treatment here recommended is the same as that practised generally, with admitted benefit, in the lowest forms of typhus fever. From this one should infer that there is no essential difference between a fever and inflammation, which every one's experience contradicts. It was my lot very early in my practice to be called on by my senior consultant to bleed

an apparently strong man in fever. After taking about four ounces of blood he fainted, and I had to desist. I have often bled in pneumonia, and the patient invariably expressed a sense of relief—for the moment at all events. If the two diseases differ so much in their tolerance of depletion is it not likely they would differ equally in their tolerance of stimulants. Some years ago I was attacked with erysipelas. The disease we know to be debilitating, and I was not in strong health previously. My kind physicians ordered me to take some claret, but had reason to repent it on their next visit, and withdrew this mild stimulant. I am quite sure if I had been an hospital patient I should have taken twelve ounces at least in twenty-four hours, whatever might have been my feelings, provided this squared with the views of the attending physician. I have always considered that the best way to learn the treatment of disease is in consultation at the bed side of private patients. In hospitals men who are engaged in founding a new school of medicine have many temptations to prevent them from deviating from their written or expressed opinions in any particular case. Suppose for a minute that it is necessary an increased supply of blood should be kept up during inflammation, is wine the best for that purpose, or is not a wine-glass of milk capable of producing more blood, when digested, than the same quantity of wine. If we want stimulants we give wine, if nutriment, broths, or milk, or farinaceous food; certainly no one that I am acquainted with thinks of giving chops or beef-steaks during the existence of acute inflammation; still young practitioners will be inclined to think differently from reading the opinions of eminent writers. I have never been able to divest myself of the idea that in all acute diseases Providence has arranged for our safety in the best manner, and would not have so invariably removed the appetite in these diseases if a large supply of blood were necessary for obtaining a cure. Nature says you shall not eat. The doctor says you shall. Nature says you must have cold drinks. The doctor says you must have brandy. Which is to be obeyed? One more topic in connexion with this important subject I shall refer to. It is quite clear that bleeding and severe antiphlogistic treatment is not so much practised in this disease as in former times; and some assert that the character of the disease has become changed. Against this it is asked, is the whole constitution of men so changed that their diseases should partake of the alteration? Should not a wound received at the battle of the Alma undergo the same treatment as a similar wound received at Waterloo? But I think this question is wide of the point at issue. The character of many of the diseases from which we suffer depends on causes external to ourselves. For instance, the last epidemic of scarlatina that visited this city was different in many respects from the same disease occurring at other times. We know that erysipelas will affect hurts and wounds at one time which would not touch them at another; and every one

acquainted with disease on a large scale knows that pneumonia is sometimes absent from our hospitals for some years and then occurs with great frequency. Of course this must be owing to atmospheric causes, which if they are capable of producing the disease may produce it in variously modified forms. For my part, I rarely see cases of pneumonia in hospital till the stage of exudation has passed, and the restorative process has commenced. At this period no one would think of antiphlogistic treatment, which would be like destroying the furniture of a house with water after a fire had been extinguished. When I met cases in private practice in the early stage of the disease, I found the same advantage from bleeding or copious leeching as I did in my early days. I have also met cases both in and out of hospital which required stimulants from an early period, owing to the constitution and condition of the patient, irrespective of the state of his lungs. With reference to bleeding I hold very moderate opinions. If a physician chooses not to practise it he cannot be said to have done his patient harm; at most he has only withheld good from him. It is different with the giving of stimulants, which is a positive act for which he is responsible. In conclusion, the danger I apprehend to the profession from statistics is this—that assumed results would lead the young practitioner to think there is some royal road to the treatment of disease, and that he has only to make a correct diagnosis, and leave the nurse to administer the prescribed amount of stimulants and nutriment. Whereas it will be for ever the lot of the conscientious physician to tread his way cautiously in every case. To shut out from his mind all preconceived opinions, personal vanity, and self interest, and to treat each case on its own merits, and solely for the good of the patient.—*December 13, 1865.*

Albuminuria ; Gall-stone ; Suppression of Urine. By DR. FINN.

John Heffernan, a butcher, aged fifty-nine, of intemperate habits, was admitted into the North Infirmary on the 16th November, 1865, suffering from abdominal pain, costiveness, and irritability of stomach, which symptoms had been in existence for a few days previously. He had been an intern patient of the North Infirmary about two years since, labouring under obstruction of the bowels, on which occasion his convalescence was exceptionally tedious. During the past Autumn he had been treated with benefit in the South Infirmary, for anasarca, complicated with renal disease.

November 17th.—Costiveness and irritability of stomach persist; face and body generally jaundiced; occasional hiccup; pulse scarcely perceptible; urine very scanty, and deeply tinged with bile. On applying the hand over the abdomen, even with gentle pressure, great pain is experienced; but the pain presents its maximum intensity at a point corresponding with the free edge of the liver, where the gall-bladder is

situated. On the occasion of this visit an opinion was expressed that the jaundice was caused by a gall-stone, which occupied the ductus choledochus.

November 19th.—Costiveness slightly relieved by enemata, but the alvine dejections are destitute of bile. Complete suppression of urine. Death took place on the 20th instant.

Autopsy.—The viscera (both solid and hollow) were deeply stained with bile. The peritoneum was thickened and congested. The liver was enlarged and altered in form, the free edge being rounded off. A gall-stone of large size was found to occupy the commencement of the ductus choledochus, rendering the escape of the bile into the duodenum mechanically impossible. The form of the gall-stone was that of a drum; it measured two inches in circumference, and three-quarters of an inch in the vertical direction; several small gall-stones were also found, with a large quantity of cholesterine in the granular form. A section of the larger gall-stone presented a “nucleus,” of considerable consistence, the portion intervening between it and the surface being quite granular and easily broken up: the superficial layer also possessed a moderate degree of consistence. The kidneys were hypertrophied and congested, well exemplifying that form of albuminuria which is characterized by morbid development of the structures in question.

Remarks.—The determination of bile to the kidneys, consequent on the occlusion of the ductus choledochus as already noticed, would appear to have overtaken the remedial activity of those organs, and thus concurrently with the hypertrophy of the renal structures to have induced suppression of urine, which was the more immediate cause of death.—*November 22, 1865.*

Epistaxis; Anasarca; Albuminuria; Hypertrophy of the Heart in the Absence of Valvular Disease. By DR. FINN.

Michael Mahon, a labourer, aged thirty-eight (respecting whose antecedents little or nothing was ascertained), was admitted into the surgical wards of the North Infirmary, on the 25th September, suffering from profuse epistaxis, which was only controlled by the plugging of the posterior nares, other means having failed to afford relief. When the epistaxis had subsided, anasarca immediately supervened, and contemporaneously with it effusion into all the cavities. When he was removed to the medical wards, his face was blanched to the last degree, and the difficulty of breathing amounted to orthopnea. His replies to questions were generally incoherent. Pulse quick, and excited. Tongue pale, quite in keeping with the pallor of the countenance. The secretion of urine was normal in quantity. Auscultation revealed nothing abnormal in the sounds of the heart. Death took place on the 14th November.

Autopsy.—Effusion to a large amount was found in the cavities of the pleura and pericardium. A good deal of superficial adipose deposit on the heart, near the base. The heart was remarkably pale, but firm in its consistence; it weighed twenty-one ounces and two drachms. The left ventricle was much hypertrophied; no valvular incompetency was found to exist. The kidneys were much diminished in size, and irregular on the surface

Hypertrophy of the heart, in the absence of valvular disease, has been noticed by Dr. Bright as frequently co-existing with albuminuria.—*November 22, 1865.*

On the Constituents of Food, and their Relation to Muscular Work and Animal Heat. By F. C. DONDERS, M.D., Professor of Physiology and Ophthalmology in the University of Utrecht. Translated from the *Nederlandsch Archief voor Genees- en Natuurkunde*, 1^e Deel, Utrecht, 1864, by WILLIAM DANIEL MOORE, M.D., Dub., M.R.I.A.; Honorary Fellow of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen; Examiner in Materia Medica and Medical Jurisprudence in the Queen's University in Ireland.

(Concluded* from page 252.)

V. RELATION BETWEEN THE PRODUCTION OF HEAT AND OF MECHANICAL WORK.

As each form of energy may be changed into heat, it is evident that each element of food may, directly or indirectly, produce heat in the body. Of mechanical work the same cannot, *a priori*, be asserted. In this case, for the possibility of origin from all elements of food, the argument is wanting, that each form of energy should be capable of giving place to mechanical work. On the contrary, we know that heat can only partially pass over into mechanical work, and that even for this conditions are required which are not present in the animal body (compare p. 243). It was thus imaginable that certain substances, on oxidation, develop only heat in all tissues, and there was, so far, nothing absurd in the supposition that non-nitrogenous bodies could produce no mechanical work, that this depends simply and solely on metamorphosis of albuminous matters. But if this theory was not absurd, it was nevertheless extremely arbitrary, and also highly improbable. Consequently, it was by no means reasonable that it should be looked upon as proved. At most, it could be looked upon only as an hypothesis. Its untenability will have appeared so soon as it shall be shown that the chemical energy of the consumed albuminous matters is not equivalent to the mechanical work

performed. We propose to investigate this. To this end we shall endeavour to discover the relation between the quantities of mechanical work and of heat developed in the body, in order to be able to compare therewith those of the nutritive elements in the food used.

Traube supplies the following calculation:—^a

A *working* horse produces 75 kilogrammeters per second, and consequently in 8 working hours 2,160,000 kilogrammeters $= (2,160,000 : 430)$ 5025 calories.

A *non-working* horse consumes, according to Boussingault, in 24 hours, 2465.1 grammes of carbon and 24.4 grammes of hydrogen, affording on combustion to CO_2 and HO about $(2465.1 \times 9.6 + 24.4 \times 44.462) = 24506$ calories. Supposing that the horse, in performing work, uses only so much more organic matter as is equivalent to the work done, Traube now finds the relation between the production of heat and of work $= 24506 : 5025$, that is about $= 5 : 1$, and for the working time, calculated at $\frac{1}{3}$ of the day $= \frac{24506}{3} : 5025 = 8.2 : 5$. Consequently, during the working time the mechanical work performed would amount to about $\frac{5}{8.2}$ of the work, represented by the heat produced.

This calculation suggests one or two remarks. In the first place, the assumption that, in work, greater consumption of matter would occur exclusively for this work, is very far from the truth. We know, in fact, that the production of heat in this case is also considerably increased, and experimentally double, nay five-fold excretion of carbonic acid has been observed. In the second place, the calculated horse-power is too great for an ordinary horse, to which the food calculated refers. We shall make a similar calculation from observations upon man, communicated, it is true, by Traube, but not employed for calculation, which will bring us to a completely different result.

Lavoisier^b and Séguin found the consumption of

oxygen by a man, not during the time of digestion, at 59° F., in the state of rest, for one hour,	26.66 litres,
During the performance of work,	63.477 „

For the increased consumption of . . . 36.817 „

an amount of work was performed equivalent to the lifting of 7.343 kilogrammes to 799 metres, that is 5867 kilogrammeters $= (5867 : 430)$ 13.583 calories. On the other hand, 36.817 litres $= 49.89$ grammes of oxygen yield, on combustion with carbon, 179.616; on combustion with hydrogen even 214.905 calories. Hence it appears, that at most $\frac{1}{4}$ of

^a Virchow's Archiv, B. xxi., page 391.

^b Conf. Gavarret. De la chaleur produite par les êtres vivants, p. 330. Paris, 1855.

the greater consumption of oxygen was used for mechanical work ; of all the oxygen consumed, not more than $\frac{1}{24}$.

In another case, Lavoisier and Séguin found the consumption of oxygen in a man, during digestion

at rest, again at 59° F. for 1 hour,	.	.	37·689 litres,
During the performance of work,	.	.	91·248 „

For the increased consumption of	.	.	53·559 „
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a work is here performed of 6201·8 kilogrammeters=14·423 calories, while for 53·559 litres of oxygen, burned in the body, 261·274 calories are obtained, so that of the increased consumption of oxygen, certainly not more than 14·423:261·274, that is $\frac{1}{18}$, and of the total consumption still less than $\frac{1}{30}$, is represented by the work performed. To these results, which would fully justify us in deriving all the mechanical work from nitrogenous combinations, we shall not attach too great importance, because we believe that the work performed in the investigations is estimated too low. But they form an admirable appendage to the calculations of Traube.

Coulomb calculates in the ascent of the Peak of Teneriffe, with De Borda, 204610 kilogrammeters per day of 8 hours, that is, 25576 per hour, or four times as much as was assumed by Lavoisier. For other work, such as the laying of paving-stones, Coulomb calculated much less, namely, 75240 kilogrammeters daily ; Lamandé, 80635 ; Haughton, 124000 kilogrammeters, which, calculating the working time at 12 hours a day, amounts to little more than one and a half times the work found by Lavoisier. If we reckon 86000 kilogrammeters=200 calories, these are obtained by burning about 20 grammes of carbon of carbo-hydrates, while the daily consumption of carbon amounts to not less than 240. Consequently, even leaving out of view the hydrogen of the fats, &c., the work performed would be equivalent only to $\frac{1}{12}$ of the chemical energy used.—Under these circumstances, too, it was still possible to admit, that only albuminous matters produce this work.

In extraordinary exertion, under which head certainly the ascent of mountains is to be classed, the work performed is greater in reference to the heat produced. Helmholtz^a supplies us with a calculation on this subject, based on the results obtained by Dr. E. Smith of London, who, with a compact gasometer, determined the quantity of air inspired under different circumstances (sleep and vigorous exercise not excepted), and, with a second equally portable apparatus, collected and weighed the carbonic acid expired during many hours. In the first place, Helmholtz deduces from the results obtained by Dulong and Despretz, from experiments on animals, how much the temperature of the human body ought to rise by

^a Lectures on the Conservation of Energy, delivered at the Royal Institution, April 1864.

accumulation of heat, developed in one hour, in the condition of rest, and for this he finds $2^{\circ} 16$ F. This heat is the equivalent of an ascent of the body to 1,760 feet. Now, by strenuous exertion, a practised climber of mountains can attain this height in the space of one hour. But in such exertion Smith finds that the expired carbonic acid is five times greater than in the state of rest, and as only one quantity serves for the ascent itself, four times the quantity remains over for the heat produced. Hence it follows, that the equivalent of mechanical work and heat, developed in active exercise, are to one another $=1:4$. In this nothing is brought into calculation for the loss of carbonic acid by the skin, which precisely on great exertion, in horses at least, is very considerable (Gerlach). For ordinary work, the proportion of $1:5$ is certainly estimated highly enough. The result is therefore quite different from the $3:5$ calculated by Traube. But this does not hinder us coming so far to the same conclusion, that we think the origin of mechanical work exclusively from nitrogenous matters inconsistent with this result.

We have here to refer to our demonstration, given in the preceding section. At the close of it (p. 252), we arrived at the result: that *in mechanical work, yielded externally, only a part of the elastic energy produced in the muscles is still found*. We remarked, that a second portion of the elastic energy—namely, what remained over in the subsequent relaxation of the muscle—must give place to heat. Evidently, however, with the elastic energy, the direct condition for mechanical work is already given. How far that will become for mechanical work depends on the loading, and is, in a certain point of view, accidental. Consumption of matter, in other words, chemical energy, is in any case no longer necessary thereto. We have, therefore, for comparison with the nutritive principles used, to know the relation between the elastic energy and the heat; and if we assume that the half of the force occurs not as mechanical work, the proportion alters from $1:5$ to $2:4$. If we consider, further, what numerous muscles come into action in each movement, without contributing directly to that movement, how much elastic energy in respiration, how much mechanical work in the circulation of the blood, and, moreover, in other functions, are changed into heat, we may for this also set down for $\frac{1}{2}$ part transition of elastic energy and muscular work into heat, and we then obtain for the original relation of elastic energy to all forms of work, that of $3:3$; in other words, about the half of the chemical energy of our food is changed, in the muscles, into elastic energy.

Now, in ordinary feeding, we may calculate for man:—500 grammes of carbo-hydrates, to 100 grammes of fat and 100 grammes of albuminous matter. In general, these last will represent not more than $\frac{1}{2}$ of the chemical energy. If, therefore, all albuminous matters were employed for muscular work, which is by no means the case, they would be

insufficient completely to explain this. It is, therefore, certain that, besides the albuminous matters, there are constituents in our food from the use of which elastic energy is produced in the muscles.

VI. NITROGENOUS PRODUCTS OF METAMORPHOSIS OF TISSUE.

In Traube's theory we see the most radical reaction upon the doctrine maintained by Liebig. If, according to the latter, mechanical work could be produced only from albuminous matters, Traube inverts the proposition, and asserts that in such work exclusively non-nitrogenous substances are used, upon the oxidation of which the nitrogenous are said to have only an indirect influence. In presence of this assertion, our task is quite different. If we had, above, to prove that not all mechanical work takes its origin from nitrogenous matters, we must now examine whether any direct share therein belongs to them. The solution appears to be attainable by determining the excreted matters. By comparative investigation of these, both qualitatively and quantitatively, in the conditions of rest and of work, it may certainly be found, what matters in the latter case are used in greater quantity. Now it is known that in work much more carbonic acid is excreted. We may further assume, that in proportion herewith the formation of water in the body will be increased. But, on the contrary, according to the investigations of Voit, the eliminated nitrogenous compounds are not essentially increased. Great weight is attached to this result. It has led Traube to apply his fermentation theory to the metamorphosis of matter in the living muscle. Now, have these investigations actually proved that in muscular work the metamorphosis of nitrogenous bodies is not increased?

As is well known Bischoff and Voit assume, that all the nitrogen disappears from the body by the kidneys and the intestinal canal—in the dog almost exclusively in the urea of the urine. In the first place, we have to inquire how far this proposition has been proved.

The question, whether nitrogen is excreted by perspiration, always presented great difficulty. If, on analysis, the nitrogen of the expired air was found to be greater than that of the inspired, it was very evident that this did not prove the fact of an absolute increase: the diminished volume of the air might equally account for it. An attempt was now first made to answer the question indirectly. If the absolute quantity of nitrogen, both in the food and drink, and in the feces and urine, were determined, the difference ascertained must represent the quantity of nitrogen which has escaped in the perspiration. Boussingault found this to be in the cow not less than $\frac{1}{6}$; in the horse more than $\frac{1}{7}$; in the turtle dove, $\frac{1}{3}$ of the nitrogen of the food. Barral, in man, found it to be about one-half; Sacc, in chickens, still more than one-half. On the

* Art. Harn. Wagner's Handwörterbuch f. Physiologie.

other hand, C. G. Lehmann^a had, under the use of highly nitrogenous diet, recovered nearly all the nitrogen of the food in the urea, while Jørgensen, in experiments continued for a month upon a sheep, found in the feces and urine 93 per cent of the nitrogen. Lastly, Bidder and Schmidt had, in some experiments upon dogs and cats, recovered nearly all the nitrogen of the food in the urea. The question now attracted the attention of Bischoff. He applied to Liebig for a volumetric method of determining the urea of the urine, and Liebig gave him, in the solution of nitrate of mercury, a fluid which answered his requirements beyond expectation. According to Voit, in fact, not only the urea is precipitated by this fluid, but also the other nitrogenous constituents of the urine, and indeed almost in the inverse ratio of their nitrogenous element; so that what the boldest imagination had not dared to expect was ascertained, the amount of nitrogen in the urine, no matter in what combination it might exist.

Meanwhile, Bischoff found at first not nearly all the nitrogen of the food in the urine; and it was therefore rather strange that he, on this ground, sought the measure of the metamorphosis of tissue in the quantity of urea excreted by the urine. The investigations of Voit, which soon after followed, appeared rather to prove this measure to be acceptable. But even in them, too, the nitrogen of the urine and feces, after correction for the altered weight of the body, always remained below that of the meat used, notwithstanding that only the minimum of the nitrogen found therein was taken into account,^a and I therefore do not hesitate to assert, that neither did these experiments exclude the possibility, that about 10 per cent. of the urea had escaped in other ways. Haughton now finds, moreover, in man, in contrast to Barral, nearly all the nitrogen of the food in the urea and feces. Likewise Ranke, in a series of investigations which bear the best stamp; so that Barral's result is to be considered as refuted. In horned cattle, too, Henneberg found the deficit in general less than that of Boussingault; where the supply of nitrogen was small, the deficit was comparatively great—thus also there was a deficit, under circumstances in which an addition of flesh in the animal was not to be assumed. In his subsequent investigations Voit obtained, not only in four dogs anew nearly all the nitrogen of the food from the urea, but also in a pigeon which he (in order to test Boussingault's results) had fed for 124 days

^a Voit found in fresh meat, as maximum, 3.73 per cent., as minimum, 3.41, and as average, 3.59 per cent. of nitrogen, and brought into calculation only the minimum; that is, 5 per cent less than the average and 9 less than the maximum. In the calculation, too, of all subsequent experiments, he has kept to the minimum. The reason he assigns for this is, that the meat analysed was much more accurately freed from adipose tissue than that intended for food: it is, indeed, not difficult to free meat from adipose tissue to less than 1 per cent., and I believe that Voit will certainly have done this.

with peas, containing a known amount of nitrogen, the quantity of that element in the excreta being constantly determined; no deficit worth mentioning (1.1 gramme) was, after correction for a slight increase of weight, met with. Finally, an experiment upon a bull, detailed, at length, by Grouven^a, is in favour of the elimination of nearly all the nitrogen in the feces and urine. This writer, now assumes in all further investigations and calculations, that no nitrogen escapes with the perspiration, though not, as he expressly says, because he is as fully convinced of the impossibility thereof as Bischoff and Voit, but—because otherwise the results of his investigations would not be of much use. Grouven has also convinced himself that only traces of ammonia escape, which are not taken into account: indeed, the debates upon this subject are at an end.^b

But is it now really certain that all the nitrogen is excreted by the kidneys and the intestinal canal? that not a trace of nitrogen disappears through the lungs or skin?

To the results of indirect experimentation above detailed those of direct investigation are strictly opposed. I allude to those of the investigations of Regnault and Reiset, continued for years by Reiset, and engaged in on a smaller scale also by Marchand. Regnault and Reiset caused animals to breathe in a closed space for about 24 hours, while, by a peculiar mechanism, the carbonic acid formed was constantly absorbed, and was replaced by an equal volume of oxygen. Where they found, at the end of the experiment, the absolute quantity of nitrogen in the enclosed space increased, they inferred that nitrogen was excreted by perspiration. Voit made it appear, as if Regnault and Reiset found the nitrogen at one time increased, at another diminished, without any assignable cause, or any regularity. In this he is deceived. The experiments show that, in warm-blooded animals, the quantity of expired nitrogen is usually less than $\frac{1}{100}$, often amounts to only $\frac{1}{200}$, and never exceeds $\frac{1}{30}$ of the oxygen consumed. But only under particular circumstances, namely, after the withholding of food, almost without exception in birds, sometimes also in mammalia (in marmots during hybernation), was the nitrogen, in place of being increased, found to be diminished. Reiset continued these investigations for years, and his results communicated last year to the Institute, of experiments upon sheep, calves, and wild boars, are similar to the former conclusions. Marchand, too, found for 100 volumes of carbonic acid 0.94 volumes of nitrogen expired.

All these investigations were blotted out by Voit with a stroke of his pen. We may regret with him that Regnault and Reiset appear to have

^a Physiologisch-chemische Fütterungs-Versuche. Berlin, 1864, p. 121.

^b Conf. Kühne and Strauch. Centralblatt f. d. mediz. Wissenschaften, 1864, Nos. 36, 37.

neglected to control the closing of their apparatus. But has he the right, therefore, to set aside these results without having repeated the experiments in a controlled and correct apparatus, with a negative result? With such experiments he would much better have silenced his adversaries, than with a philippic on their obstinacy. It is evident that in the controversy facts are arrayed against facts. We shall not defend the results obtained by earlier investigators against those that are more recent. We acknowledge that in the force of some results of indirect experimentations there is little to object to. According to the experiments so long continued by Voit upon one and the same pigeon, we must believe that a pigeon can live without losing nitrogen, otherwise than by the feces and urine. But all the other experiments, even those of the more modern observers—leaving out of consideration the frequent deficit of nitrogen—admit the possibility, partly on account of the short duration of the experiments and the impossibility of bringing into account with perfect accuracy the change of bodily weight—partly on account of the uncertain amount (probably calculated too low) of nitrogen in the meat used—partly (in horned cattle) on account of the very variable contents of the intestine (Grouven)—that some nitrogen, as such, disappears through the perspiration. These experiments can, consequently, not refute the existence of this excretion; and with our eye upon the results of the direct experimentation, we are not at liberty flatly to deny it.

VII. INFLUENCE OF MUSCULAR WORK UPON THE PRODUCTS OF METAMORPHOSIS OF MATTER. OBSERVATIONS OF VERLOREN AND OF VOIT.

In the first place, I may here communicate the observations of Dr. M. C. Verloren. They were made and communicated in our circle, "*amicitia naturæ interpres*," at least two years before the investigations of Voit were known, and before Traube established thereon his theory. On my requesting that he would be good enough to give me, in writing, the leading points of his statement, I had the satisfaction to receive the following answer, which will certainly be interesting to my readers:—

"The observations upon insects, which had already led me to the conviction that muscular work is attended with no considerable metamorphosis of albuminous matter, consist in the following:—

"1°. Many insects use, during a period in which very little muscular work is performed, food containing chiefly albuminous matter; on the contrary, at a time when the muscular work is very considerable, they live exclusively, or almost exclusively, on food free from nitrogen.

"2°. During the first period a very great quantity of urine is excreted, in proportion to the respiration, and the respiration also is absolutely little; during the second period, on the contrary, the urinary secretion is

slight in proportion to the respiration, and the respiration also is very considerable.

"Of the first proposition I formerly quoted bees as an example, and they have since been indicated also by others. But as I do not know whether this argument has been brought forward in its full force I shall say something on the subject.

"Bees have three sorts of cells: 1°, for working bees; 2°, for male bees; 3°, for queens. The first are by far the most numerous, the smallest, and are of the same form and capacity as those in which the honey is collected and kept; the second are somewhat larger; and the third are still larger and of a totally different shape. In the bottom of each of these cells the queen lays an egg. After some days a worm-like larva emerges from the latter, lying on the bottom of the cell. The working bees then daily bring these larvæ food, which they pour out into the cells. This the larvæ appropriate, become larger, and at last fill nearly the whole cell; they are then full-grown, and spin a dense web, closing the openings of the cells from above. They then no longer receive any food, but undergo this change, whereby a completely new being is formed out of the accumulated food, with totally different organs and tissues. When the bee is fully formed, it gnaws away the spun covering, and comes out. It now enters upon its active period of life, in which, during a considerable time, it performs enormous muscular labour in its numerous and very rapid flights, in its great industry and vivacity; bees are, in fact, the most restless and most lively of insects. In the larva state, on the contrary, their muscular work is as slight as possible, being confined to swallowing the food, turning occasionally in the cell, and spinning the closing material of the same.

"Now, of what does the food of bees consist? Of the so-called *bee-bread*, that is pollen from the flowers, with a little honey; and, 2ndly, of honey. The first, containing a large quantity of albuminous matters is exclusively the food of the larvæ; the second of the full-grown bees, which use very great quantities thereof, as is evident from the considerable amount of it which they accumulate, to serve as a store during the Winter, and when from other circumstances, they cannot go to fetch it. In the Winter there are no larvæ.

"I have already referred to the butterflies. The caterpillars, it is true, develop more muscular work than the larvæ of bees; they must themselves, crawling, seek their food, must spin more, or crawl more, to seek for a suitable place for transformation to the chrysalis state. Still this is only inconsiderable when compared with the action of the restless flying butterflies. Now, the caterpillar feeds on leaves containing albuminous matter; but the flying butterfly seeks only honey from the flowers; their mouths are even not adapted for the reception of any other food.

"I now pass over to the second point, from which alone the first derives its full force, and to which my investigations have more decided reference.

"During the larva condition there has been formed from the food consumed, in addition to the organs of the caterpillar or larva, a thick layer of adipose tissue, consisting of albuminous matter and fat, situated beneath the skin with its muscles, and surrounding the intestines. In the chrysalis state this adipose tissue again becomes fluid, and is taken up into the blood, as are likewise the existing organs of the caterpillar, and out of these materials totally new organs and tissues are formed, producing an entirely new being, the perfect insect. During this transformation no more food is taken in, but what is already present in the animal is quite unmixed and ample. Neither are any excrementitious matter formed and voided; the respiration alone continues, so that the chrysalis constantly decreases in weight. In this metamorphosis of the organs, and in the course of the other vital functions of the insect, urine is, at the same time, in connexion with the respiration, secreted; this, however, is not evacuated, but collects in a great vesicular process, which arises at the rectum, in the formation of the organs of the perfect insect. Immediately upon, and shortly after leaving the chrysalis envelope, the perfect insect expels the whole quantity of urine formed and collected during its chrysalis existence.

"I must also make mention of the prolongation beyond a year of the chrysalis state, a phenomenon which takes place in some insects. Most insects are in their different states bound to definite periods of the year. Now, it happens in some insects, that when the time for the chrysalis to emerge is past, some have not come out; these, then, lie over for another year, in order then to emerge at the same time as those which did not pass the year. In *Sphinx Ligustri* this often takes place. It is to that insect that the following results refer. It remains about 300 days in the chrysalis state, so that those which pass the year, continue in it for 665 days. The investigations were made on a certain number of insects, varying for the different investigations, but separate for each individual. The result here given is the average of the several observations.

"The following chrysalis have thus, during the chrysalis state, given, in urinary secretion and respiratory loss, calculated in per centage of the weight of the chrysalis on the 66th day before the emergence of the butterfly, as follows:—

		Urinary Secretion.
Annual,	♀ ♀	45.89 per cent.
"	♀ ♀	31.76 "
Biennial,	♀ ♀	35.64 "
Further urinary secretion for the additional year,		3.88 "

			Respiratory loss.
Annual,	.	.	10.71 per cent.
"	.	.	10.43 "
Biennial,	.	.	10.40 ,, respiratory loss in 300 days.
Further urinary secretion for			
the additional year .	7.74	,,	respiratory loss in the additional year,

"The relation of the urinary secretion to respiratory loss in females for one year, and for the extra year in the biennial females, is therefore:—
For 100 urinary secretion, 32.75 respiratory loss in those of one year.

" " " " 199.48 " " for the extra year.

"The butterflies have yielded in urinary secretion and respiration, per hour, in milligrammes:—

	♂	♀
Urinary secretion,	0.326	0.425
Respiration,	1.253	1.252

"For the butterflies I cannot easily state the quantities in per centage upon the bodily weight, but for the relation between the urinary secretion and respiration, which is here principally in question, this is not necessary. We see that this is about as follows:—

Chrysales.				Butterflies.	
Annual,	♂	Annual,	♀	Biennial,	♀
				Extra year.	
4	3		$\frac{1}{2}$	$\frac{1}{3}$	$\frac{1}{4}$ Urinary

secretion to respiration, so that in the male chrysalis fifteen times more urine is excreted than in the male butterfly.

"The greater urinary secretion in the male chrysales than in the female, is constant. Now, the chrysalis performs scarcely any muscular work; the butterfly an enormous amount."

"When we take into account that the ♂ chrysalis excretes nearly half its weight in urine, when we add thereto what is lost in respiration in the chrysalis envelope and in the exhalation of the butterfly on emerging, which is very considerable; when we, furthermore, reflect that the butterfly consists only in small part of muscles, and that it takes in *no more albumen whatever*; it cannot be otherwise than that for muscular work but very little albumen is metamorphosed."

"As the butterfly does not weigh the half of the chrysalis, and nearly the half of the chrysalis passes into the urinary secretion, the whole butterfly would necessarily be wasted away if it would consume as much albumen as the chrysalis. But the muscles and the albumen still present, which might be used for muscular work, will not amount to a fourth part of the butterfly. If, therefore, the butterfly entirely consumed its muscles, it would have used only a fourth of the amount of

albumen used by the chrysalis. But muscles, which are no more cannot perform any function, so that only a very small portion of the muscle can be consumed if it is still to remain in a serviceable state. I will assume a tenth, in which case the butterfly has at its disposal only a fortieth part of the quantity of albumen consumed by the chrysalis. And yet the butterfly performs muscular work, and the chrysalis almost none.

"It seems to me that it is as clear as possible that the albuminous matters exclusively, or at least almost exclusively, are metamorphosed in the formation of the tissues and organs, and that the non-nitrogenous nutritive matters, by their disintegration, produce work.

"I will yet remark, that the observations on the butterflies deserve the least confidence, but that in more accurate essays they would tell still more to my advantage, that probably the respiration will prove to be still greater, and the urinary secretion as much less. Also that the first three respiration numbers in the chrysales must really be somewhat higher, but too little so to have any influence upon the results."

In heartily thanking my friend Verloren for the communication of his investigations, which are as important as they are accurate, he will not take it ill of me if I bring under the eye of our readers my reflections, which are well known to him, against the inference he has drawn from them. That the larvæ, in their development, that is, in the building up of albumen-containing organs, take in albuminous matters in their food, is not to be considered strange. Out of albuminous matters the young bird in the egg is also principally developed; and the embryo of the mammal, which, except the movements of the heart, certainly performs very little work, needs for its development also chiefly albuminous matters. When the larva is full-grown, a period of quite peculiar metamorphosis ensues. That then, while from the old a new being is formed, many albuminous matters should be metamorphosed, cannot surprise us. The phenomenon simply proves that metamorphosis of albuminous matter exists without any connexion with movement, and on this point there is no room for doubt. From the experiments of C. G. Lehmann and Hasse we have already learned, that feeding with highly albuminous matters, independently of muscular work, considerably increases the excretion of urea, and in the excreted urea we can consequently find no measure of work performed. Further, it appears inadmissible to compare the consumption of matter of the perfect insect with that of the chrysalis state, with which such peculiar nutritive changes are bound up. That consumption of matter must be considered by itself; and then the question is only, how much muscular work the insects perform, and how much nitrogenous and non-nitrogenous matter is at the same time metamorphosed. And as to this work: by the remark, that the animals are quick and lively, and perform long

journeys, it is not measured, as little as all nitrogenous products of the metamorphosis of matter are determined by the excreted uric acid. Lastly, I am somewhat sceptical as to the composition of the food taken in. Does the butterfly suck so exclusively non-nitrogenous honey out of the flowers? Precisely in Winter, when the bees do not stir, this non-nitrogenous honey appears to be sufficient. Undoubtedly, the facts collected by Verloren are important; they show, that in insects much albuminous matter is used, and much uric acid formed, without the intervention of movement. They shed light upon the metamorphosis of matter connected with the several stages of insect life. But to the question, how much nitrogenous matter is consumed in reference to the equivalent mechanical work they give no satisfactory answer.

We now come to the investigations of Voit. They are confined to the determination (by Liebig's volumetric fluid) of the amount of urea contained in the urine of a dog both during abstinence and during regulated feeding with meat, in both cases with and without the performance of mechanical work.

The work which the dog had to perform consisted in running in a tread-mill. Voit calculates this (indeed somewhat liberally) at 150,000 kilogrammeters daily. The observations extend over four series; 1°, one of five days, during the withholding of food; on the second and fourth day with work, 16.6 grammes of urea were excreted; on the three other days, in a state of rest, the amount was 14.3 grammes. 2°, one of nine days, food being likewise withheld, on the fourth, fifth, and sixth days, with work, 12.33 grammes were excreted; on the other days, the average amount was 10.88 grammes. 3° and 4°, two series, on each of three days' work, followed by three days of rest, begun after an equilibrium of ingesta and egesta had been attained by regular feeding with meat; on the days of rest the excretion amounted on an average to from 109 to 110 grammes; on those of work to 114, when the labour was performed before eating (3rd series); to 117, when it was performed after eating (4th series).

These are briefly the facts. They lead to the conclusion that during work the metamorphosis of nitrogenous matter in the muscles is not increased. Theoretical considerations connected therewith, to which Voit himself will certainly no longer attach much value, we leave out of the question. It is with the direct conclusion we have to do, and against this we have some difficulties to bring forward.

In the first place we observe, that on the days of work actually somewhat more urea was found, which Voit, without sufficient proof, thinks is to be ascribed to the increased excretion of urine. A second difficulty relates to the method followed. Does the volumetric fluid really show the amount of nitrogen present? Besides urea there are several nitrogenous matters in the urine; and Voit thinks he has satisfied

himself that the amount of nitrogen in these also is approximatively shown by the volumetric fluid—so soon as one or other of these matters, under the extraordinary circumstances of violent muscular work, is disproportionately increased, we can certainly no longer rely upon the method. Now, to deny, *à priori*, the possibility of such an increase, because even in the state of rest also a number of muscles are active,^a appears to me presumptuous; and, looking to the results of experiments obtained on other animals with respect to hippuric acid, inadmissible. I, therefore, with Meissner, think it desirable, in such experiments, to determine the amount of nitrogen directly. Our third difficulty is, that Voit feels himself justified in neglecting the loss of urea by the skin, and in denying the loss of nitrogen by perspiration. How great the amount of urea of the sweat in work may be, we know not. But in a dog, which wearies itself in the highest degree by work, it may come into consideration. Remarkably enough, Speck found in a man, that the urea of the urine was not increased only when the work was accompanied by copious perspiration. And as to the escape of nitrogen in the perspiration, we have in the preceding section shown that the results of the direct investigation are not satisfactorily refuted by those of the mediate. Now, if there is such a loss in the state of rest, there is every reason to assume that in muscular work it will be considerably increased. Even for these reasons we think that the conclusion, that in muscular work the metamorphosis of nitrogenous matter is not increased, is liable to doubt. But there is more.

What is the origin of the urea, determined by Voit. Is it wholly in the muscles? Certainly not. For the greatest part, at least? This, too, is improbable. In the nutritive fluid of muscles we find creatin, creatinin, and inosinic acid, no urea; and how far the substances mentioned are changed into urea, has not been ascertained. In every case, too, creatin and creatinin occur in the urine; and no one has shown that these should not be met with in greater quantity in muscular work. Evidently, therefore, the metamorphosis of nitrogenous matter in the muscles might be considerably increased without much of it being discovered by the volumetric method applied to the urine. It should not be forgotten that, independently of muscular work, much urea is produced—the more so, in proportion as more albuminous matter is supplied to the body; and in order to establish the increased consumption in the muscles, it is therefore certainly not enough to compare the ascertained increase of nitrogenous products of the metamorphosis of matter with the sum total of these products. It should further be borne in mind, that a greater metamorphosis during work, is followed by a slighter change during rest (Speck), and that this subsequent diminution

^a Conf. Voit, in Liebig's Annalen. 1863. II. Supplement-Band, p. 376.

might conceal the greater consumption of nitrogenous matter during work.

Lastly, we have to refer to a singular result, to which Voit's theory would lead us. It is proved that the production of carbonic acid increases very considerably in muscular work. In a good walk, in which from two to three English miles (three miles are nearly an hour's walk) are performed in the hour, Smith found the quantity of carbonic acid expired from $1\frac{1}{2}$ to $2\frac{3}{4}$ times greater than in the state of rest; in ascending a mountain it was five times greater; and when seven miles were accomplished in the hour the quantity of carbonic acid was multiplied seven-fold. Now, supposing that a dog, on pure meat diet, daily performs work, by what other matters can the increased quantity of carbonic acid be permanently supplied if not by the meat consumed? And if the nitrogen were not at the same time excreted, and indeed in the same proportion, should we not finally have the prodigy of a dog existing on pure nitrogen? This is what is called an *argumentum ex absurdo*. It may at least prove that experiments such as those of Voit, in order to lead to certain results, must be continued much longer on the same animal.

VIII. METAMORPHOSIS OF MATTER IN THE MUSCLES.

Not only the excreted products of the metamorphosis of matter, of which we have been speaking, but also the products in the muscles themselves may throw light upon the matters consumed in work. That the consumption during work is more active than in the state of rest is easily proved. In the first place, it is already included in the mechanical work produced together with increased warmth in the muscles themselves, which can have no other origin than the chemical energy of the matters consumed therein. But, moreover, it appears directly in the examination of the outflowing blood. Already Bernard had observed, that in contraction of the muscles this blood is particularly dark; and Ludwig and Sczelkow^a once found only 1.3 per cent. of oxygen remaining therein—only about the third part of the average quantity in the state of rest. Remarkably enough, the volume of carbonic acid obtained was sometimes greater than that of the oxygen which had disappeared. The question now is, what matters were here used? If the proportion of oxygen which has disappeared, and of the carbonic acid obtained, just mentioned, be established, we should be readily inclined to place the oxidation of carbo-hydrates quite in the foreground. But even then the hypothesis would still be required that a part also of the oxygen of the carbo-hydrates should have served in the production of carbonic acid, with the simultaneous development of a compound abounding in hydrogen.

^a Sitzungsberichte der Wiener Academie. 1862, B. xlv., p. 171.

But we should hesitate to take refuge in such a supposition, although Pettenkofer and Voit, as is well known, in the calculation of excreted carbonic acid and absorbed oxygen (the latter, however, having been very indirectly determined) in dogs, fed partly with sugar or starch, arrived at the same unexpected result. But we certainly admit that sugar is consumed in the muscles. A small quantity of sugar actually exists in the muscles (Meissner), and sugar is also constantly conveyed with the blood. The same is true of the fats. The muscular tissue itself contains no inconsiderable amount thereof, and the blood incessantly supplies fats. It is true that it is conceivable that both fats and carbo-hydrates may be formed in the muscles themselves, from albuminous matters (Meissner assumes this of the sugar found to exist there); but with the great quantities of mechanical work and heat, which in fatigue of the whole body, may be supplied by some few muscles, without alternation with rest, the necessity of the supply of matters from the blood, during working, is to be admitted. In the never-resting heart it can even not be otherwise.

Neither has the examination of the nutritive fluid of the muscles been neglected. Helmholtz began with a comparative determination of the extractive matters, and found after contraction those soluble in alcohol increased, those soluble in water diminished. The lactic acid of the muscles has become important, especially from the investigations of du Bois-Reymond, who proved that it is formed in greater quantity in contraction, so that even during life an acid reaction may predominate just as occurs in all muscles after death, when the nascent lactic acid is no longer neutralised and carried away by the blood. From what matters the lactic acid is formed in the muscle is not shown. Perhaps the fact that in dogs the acid reaction during life occurs with great difficulty, while in rabbits it takes place very readily, may be brought into connexion with the difference of the alimentary principles used.—It is further known that here, too, nitrogenous products of metamorphosis of matter are developed, among which creatin and creatinin occupy a prominent place. Bearing in view Traube's theory, it was certainly an important question, whether these are produced in greater quantity during the action of the muscles. The investigation carried on by Sarakov, under the guidance of Kühne, has now answered this question in the affirmative; acid muscles are found to be richer in creatinin than alkaline muscles are; and after tetanisation the amount of creatin and creatinin was sometimes more than doubled.^a Even many years ago, Liebig and Gregory found^b in the flesh of a fox, which had been for 200 days well fed in his kennel, and had become fat, not the tenth part of the creatin met with in the flesh of one shot in the chase.

^a Virchow's Archiv, xxviii, p. 544.

^b Ann. der Chemie und Pharmacie, B. 74, p. 100.

The inference to be deduced from all this is evident: nitrogenous matters, too, are, during the action of muscles, metamorphosed in increased quantity.

In the metamorphosis of matters lies the cause of the fatigue of the muscle. The principle agent herein is certainly the accumulation of the products of the metamorphosis of matter in the organ. We have above (p. 248) already shown, that tonic tension of a muscle, without any external work, very rapidly produces fatigue, in consequence of the impeded removal of the products of metamorphosis of matter. The investigations of Ranke^a fully corroborate this. They show that the removal of these matters is sufficient to remove fatigue, while injection of these matters (the extract of fatigued muscles, or even of lactic acid alone) suffices forthwith to elicit the phenomena peculiar to weariness, which then again give way on subsequent neutralization. The significance of the acid reaction for fatigue is confirmed also by the fact that in the never-wearied heart this reaction is absent during the whole of life—without doubt in consequence of conditions favourable to removal. But, on the other hand, we must not overlook the truth, that the supply of matters to the muscle is also a condition of its action. The fact that fatigue after work is more slowly recovered from than that after simple tonic tension, though also continued to total inability, may very well be connected with the need of new matters. In fact, it seems not possible that the removal of the existing products should require so much time as that during which the fatigue lasts. It is certain that the muscle has by continued action become in many respects another organ, in which the metamorphosis also is not yet equal to that occurring in a state of rest. This change of the organ, the altered composition especially of the nutrient fluid, must have a many-sided influence on the interchange of its constituents with those of the blood. That on the occurrence of the acid reaction the supply of albuminous matters will be limited, is in an ingenious manner brought by Heynsius into connexion with the periodicity of many vital phenomena.

IX. APPLICATION OF THE THEORY.

For practical application the theory is of little importance. We must be modest, and acknowledge that respecting the influence of muscular work on the metamorphosis of albuminous matters, we do not know much more than might in all strictness have been assumed since the investigations of C. G. Lehmann. By these it had already been shown, that with the use of a great quantity of albuminous matters the urea of the urine is considerably increased, and that in muscular work

^a Archiv f. Anatomie, &c. By Reichert and du Bois-Reymond. Jahrg., 1863, p. 422.

this increase (from 32 to 36 and 37·4 grammes) is comparatively slight. Others came to the same conclusion : in the first place, Draper, who, on this subject, even thought it necessary to resort to the excretion of nitrogen in other ways ; moreover, L. Lehmann, who met with increase only in a boy, and in a slight degree in women, not in men. In Speck's experiments the increase was somewhat more considerable. But in all we arrive at the result that, if only the ascertained excretion of urea be taken into account, in comparison with the augmented production of carbonic acid, the metamorphosis of non-nitrogenous matters in muscular work increases in a much greater degree than that of the nitrogenous. We readily admit this. We believe even that, as all excretion of nitrogen might have been taken into account, at least in experiments extending over only a few days, the augmented consumption would still have been found to be greater in the non-nitrogenous matters : for these few days of work fat of the body may be consumed.

But all these results bring us not a step further. They show only that non-nitrogenous and nitrogenous matters are both necessary for muscular work, as they are for life in general.

Two extremes were maintained, which appeared to be in a position to neutralise each other.

A few years ago Bischoff and Voit still held to Liebig's theory :—"It is and must continue true in all ages, that only these nitrogenous substances are the creators of force, that is, that they alone, by their metamorphosis in the animal body, give rise to effects of power, to motor phenomena. And in like manner it will remain incontrovertible, that the fat and the so-called carbo-hydrates, by their metamorphosis, produce only heat and no motor effects." And soon after Traube comes forward and asserts, with equal over-confidence :—"The organized part of the muscle is not destroyed in its work." "The metamorphosis of matter in the organized part of the muscle is neither connected with its action, nor is it increased by the latter." "In muscular action albuminous bodies in general are not destroyed." I would refer especially to the positiveness of these last words. It is remarkable that Traube erects his diametrically-opposite theory upon the results obtained by the experiments of Bischoff and Voit. The latter have now indeed given up their exclusiveness, and we soon find Traube also vacillating :^a—"My theory does not, indeed, require," he says in a subdued tone, "that exclusively non-nitrogenous substances should be employed in the maintenance of muscular action, but it requires that the excretion of urea should not increase in proportion to the muscular activity." To such a modest postulate we can have no objection. To it not a single fact is opposed. It answers any mode of feeding ; for, as the metamorphosis in the body

^a Virchow's Archiv, B. xxiii., p. 196.

never increases in so great proportion as the muscular work, even in continued exclusively animal diet, all the nitrogenous products cannot increase at all in proportion to the work performed. But I repeat, on practical ground this brings our knowledge not one step further. It does not teach us with what relation between nitrogenous and non-nitrogenous alimentary principles an animal or a man is best adapted for muscular work, and this was the problem we had to solve.

Of one thing we may be convinced. What we give in the food, and what maintains the body in a comparatively healthy state, will also be used in muscular work. If we give more albumen, the metamorphosis of albumen increases: if we give more fats and carbo-hydrates, the consumption of these in the muscles must also increase. In the greatest differences in diet, in fact, so much work is performed, that in the one case the nitrogenous, in the other the non-nitrogenous matters of the food taken in are undoubtedly insufficient to explain all the work, and they must consequently in muscular work be capable of in part replacing each other. The question is not at present how far the nitrogenous matters first yield non-nitrogenous, which are then further oxidised; the fact is that the nitrogenous are consumed, and, as the increased amount of creatin in muscular work proves, in the muscle itself. It is, indeed, a well-ascertained fact, that under the most varied proportions of the alimentary principles in the food consumed, some men accomplish violent labour, others indulge in a life of ease. In the former as well as in the latter, these proportions admit of a wide range. The system accommodates itself. We were already aware, and fresh experiments of Voit have more precisely proved and explained, that the body cuts its coat according to its cloth. If much food be given it, it uses much; with a scanty supply it observes a wise frugality. This holds good of food in general, and it is applicable also to each of the alimentary principles. If to a regularly-fed dog we diminish the supply of albuminous matter, the supply is at first exceeded by the consumption; but the diminution of albuminous matters in the body, which is the consequence hereof, gradually lessens also the metamorphosis, and thus the equilibrium between supply and waste is quickly restored. On the other hand, with a more abundant supply of albuminous substance, more of the latter remains in the body, and the consequently increased metamorphosis in this case also restores the equilibrium between supply and consumption. These results, obtained by Voit in the dog, were confirmed by Ranke in the human subject. He obtained, so far as the excretion by urine and feces could show it, an equilibrium in two series of experiments, once with a proportion of nitrogen to carbon = 1 : 11, and again with the proportion of 1 : 15.

Now, what proportion is the most suitable to man for the discharge of his functions in general, as well as for the production of muscular work.

With this question we turn to direct experience. From it alone an answer is to be expected:—That in different animals, or even in different men, according to individual peculiarity, another proportion will be required, is evident, *a priori*. We are therefore to look for no definite conclusion, where only a general, though satisfactory indication can be given. As a transition to direct experience, we have first to point to a remarkable result of the experiments of Bischoff and Voit,^a who proved that in alteration of diet the amount of water in the body undergoes a modification. When a dog, after having for some time been fed with vegetable matters, got a liberal supply of meat, the water was removed from his body in streams: by the urine alone, 120 grammes (about four ounces) more were excreted than was taken in with all the food and drink together. In accordance herewith the muscles after the use of animal food were richer in solid constituents than after feeding with vegetable matters. Hence it follows, in general, that the accommodation of the animal to a certain food depends on an essential change in its body, and we attach especial value to the particular fact that animal food makes the muscles firmer, richer in solid substance, poorer in water. It might be alleged against the force of these experiments that they were performed upon carnivorous animals—the dog and the cat—that these digested vegetable food less perfectly, and that the increased amount of water in their muscles, when upon vegetable food, is to be looked upon as morbid. But this objection, the value of which we shall not here more closely investigate, in no case affects the experiments of J. Ranke,^b which led in man to a similar result. These experiments are, in fact, extremely important. It appeared that man, in proportion to his bodily weight, cannot digest nearly so much meat as the dog. This quantity was, moreover, insufficient to support the body. Under the most liberal use possible, during which digestive disturbances were not wholly absent, more carbonic acid was removed by the perspiration than the carbon of the meat consumed could yield. Consequently, some of the fat of the body must have been used, and at the same time the deficiency of nitrogen in the urine and feces was so great that the experimenter was obliged to infer that albuminous matters were deposited in the body. Under these circumstances much more water was removed than was taken in. The change effected by the increased supply of albuminous matters is consequently this, that the body becomes richer in such matters, and at the same time poorer in fat and in water.

In this, too, we recognise a certain accommodation. By the excessive use of meat the human organism evidently approaches more and more to that of the carnivorous animal, and we believe, with Ranke, that in very

^a Die Gesetze der Ernährung des Fleischfressers. 1860. [The laws of the nutrition of the carnivora.]

^b Archiv f. Anat., &c., 1862, p. 311.

lean, but at the same time healthy men, the metamorphosis of matter agrees more closely with that of carnivorous animals.

X. THE FEEDING OF WORKING ANIMALS.

In order to prove that vegetable food is better adapted for muscular work than animal food is, an appeal has been made to our beasts of burden. These belong, in fact, almost without exception, to the herbivora. But the argument, borrowed from them, has, however, no great value. It is much as if we should infer, from the use of turf and coal for warming, that fats should not be adapted for fuel. Need it be said that we choose the herbivora, in the first place, as beasts of burden, because they are more cheaply kept? In some places people came to the resolution to confine themselves in the zoological garden to herbivorous animals, not in order to make them work, but simply for economy. Other reasons might, undoubtedly, still be adduced why we do not by preference yoke lions and tigers to our carriages.

That carnivorous animals are not otherwise unavailable as beasts of burden is proved by the case of dogs. In the high latitudes of Asia and America, where they are more equally kept than any other animals, they are, as Parry informs us, used for the conveyance of burdens, and they perform, six or eight being yoked to one sledge, day-journeys of from 40 to 50 English miles, with a load of from 800 to 1,000 pounds weight.* And with us, too, dogs worked in the country in the treading wheel, in order to churn; in town, under milk and vegetable carts; and occasionally on the high roads, under dog-cars, which, heavily laden, were drawn along, driver and all, sometimes by a couple of dogs, with incredible rapidity. It is known that these dogs work best when, with bread and milk, they are fed on the refuse of meat and bones.

But if, however, herbivorous animals are more advantageous as beasts of burden, the largest in general are to be preferred. Among these are our horses, oxen, asses, and mules; also buffaloes, camels, and dromedaries. Large animals are more suitable for most work, not only because a single one can move a large load, but they are also more advantageous, because, with a smaller surface in proportion to their mass, they lose comparatively less heat, and the consumption of matter can consequently be proportionally more useful in the production of mechanical work.^b In this point of view the elephant would be the best beast of

* These and other particulars I have taken from an address by Bruecke: *Die Arbeitsthiere*. Wien, 1854. Delivered on the 30th May of that year, at the commemorative sitting of the Imperial Academy.

^b Conf. *De stofwisseling als bron der warmte van planten en dieren*. [The metamorphosis of matter as the source of heat in plants and animals.] Utrecht, 1845; as well as Bergmann. *Thierische Wärme* [Animal Heat] in Wagner's *Handwörterbuch*.

burden ; but for most work he presents practical difficulties, and since the introduction of firearms he has been disused in war.

Now, is it a matter of indifference what food these animals use? Experience has taught the contrary. Highly-bred horses need highly-nitrogenous food. Their excellent qualities are developed under the influence of such aliments, combined with exercise and care for perfect transpiration. Thus, in these horses the cubic centimetre of muscle has reached a higher labour value than in any other working animal. In order not to degenerate they now require such food and require it permanently. The Arab never lets his horse eat grass and straw to satiety. His chief food is barley, and in the wilderness he gets milk, and if great effort be required, even camel's flesh. The horses, which in the Sahara, are used for hunting ostriches, live nearly exclusively on camel's milk and dried beans. Of our horses, too, it is well known that in order to do heavy work they require more than grass and hay. Oats are necessary to give strength and activity ; and while running the food preferred for the horses is bread. I have consulted many innkeepers and coachmen upon the feeding of horses. Their verdict has been unanimously as follows :—"The oats must be in them. If they come from the farmer they are round and plump : the farmers feed well. But such horses are not fit for our use. They sweat directly. They cannot bear a long run. If they have had hay the whole time, although they then get plenty of bread and oats for a couple of days, they cannot run upon it. The oats must be in them ; they then need not eat much by the way. To feed well constantly, that is the point. Hard running depends upon the breathing. If they have stopped for a time, they must again accustom themselves to running ; but in a few days they are ready, provided they have all through had plenty of oats. Grass is not much, but it is good for the health." In these simple words of the man whose eagerness for gain had sharpened his faculties of observation, lies a nucleus of truth. In our language it signifies that horses, in order to perform hard work, must have not watery, but firm muscles. And the food which serves best to produce such muscles is then further necessary to keep them in this condition. It is remarkable that reference is so generally made to the tendency to sweating in a horse not very well fed. Perhaps this is connected with the greater amount of water in the thick rotund body. But certainly in perfect muscles the proportion of work produced to heat is also more favourable. In this, too, exercise bears its part. If albuminous matters yield dry muscles, exercise makes them red. My friend Dr. Luijten^a showed, many years ago, that the muscles have more colour the more they are used, and that their redness depends principally upon the blood contained in the vessels. One of the results of exercise

^a De musculorum rubore. Diss. inaug. Trajecti ad Rhenum, 1840.

will therefore be a better circulation in the muscles, on which not only the supply of oxygen and fuel (*sit venia verbo*), but also the removal of the products of the metamorphosis of matter depends. Strikingly correct, too, is the expression, "hard running depends upon the breathing." If the Arabian horses are the most valuable, among the English race we find the fleetest racers. They move with the rapidity of a violent hurricane (25 metres per second), and for a short time leave even the express trains of England behind them. To this, in the first place, a peculiar structure of the limbs contribute; but it depends chiefly upon the lungs, which have in a comparatively short time to exchange the oxygen for the carbonic acid formed. We feel this in ourselves. In a rapid movement, especially in rapidly ascending a hill, our breath in a few minutes becomes too short, before any fatigue worth speaking of is felt in the limbs. Horses which are accustomed daily to do a certain work, step by step, are not in a state to do the same work trotting in fewer hours of the day.

The conclusion is this, that the best and noblest races are developed and maintained upon a diet abounding in albumen, and that each horse is better adapted for work when fed upon oats, bread, and beans, than upon hay and grass.

The labour of oxen is far behind that of horses; these animals are, however, still used here and there, in the Netherlands, too, among others, in the establishments of Veenhuizen and the Ommerschans. In general they live on grass and hay; but if they have hard work to perform they get, with a more liberal allowance of their ordinary food, so-called oxen-bread, baked principally of bean-meal, and consequently rich in albuminous matters. Without this bread they fail in this work.

What we read of the reindeer is important. Brooke tells us that in one day it not unfrequently runs for nineteen hours, performing 150 English miles, yoked to a sledge of 240 pounds. This work is equal to that of four Esquimaux dogs. Now in the Imperial Menagerie at Schönbrunn a reindeer gets as his daily food 2·24 kilogrammes (nearly five pounds avoirdupois) of Iceland moss. But Brücke tells us that the food of the reindeer is not always so sparing, that in Summer he seeks the mountain pastures, and in Winter lives on the parasitic plants of the forests, and for want of other nitrogenous food catches and devours field mice.

Moreover, the accounts respecting the long journeys which camels take, laden with heavy burdens, border on the incredible. And yet it is a question whether in these journeys they perform very much work. In estimating this the chief element is, how much the animals, in moving, raise their bodies, and with them the load which they carry. I am not aware that any observations have been made on this point, but I venture to suggest that camels are indebted for their extraordinary

capability of carrying loads partly to the slight displacement undergone by the latter in a vertical direction. Of camels we know that in their large anterior stomach they can take in at once an abundant supply of food and water, and that, moreover, they have a store in their humps, the fat of which, in the absence of food, is consumed on long journeys. In other respects I do not find sufficient information on record, as to the food they get, to draw further inferences, even if we knew what work on a level the movements of their laden body represents.

On the whole, we come to the conclusion that animals, to perform work, *constantly* use not only a larger quantity of food, and therefore proportionately more albuminous matters, but that they further require for their labour a more highly albuminous diet. In this way the body attains a definite stationary condition, and that food is thus consumed and used. Consequently in the excreted matters the same quantity of nitrogen must occur as in the ingesta. The nullity of the argument derived from the slightness of the increase of the urea excreted during the performance of work is thus again made apparent.

XI. THE FOOD OF MAN.

We have approached the great question:—What food does man require? We may put this question generally. It is then not simply with what food is man best adapted for work, but rather, under what relation of the alimentary principles is he capable of the greatest perfection in mind and body. If science can here pronounce no definite opinion, neither has experience solved the problem. Where we, whether in the same people or in people of like race, meet with a great difference, in general not only is difference of food connected therewith, but contrasts exist in many circumstances of a material and moral nature, and our knowledge fails to isolate and estimate the influence of each circumstance. We are, however, not without a leading principle. One fact is established, to which we attach great importance, that man everywhere exhibits a tendency to the use of mixed food, and under its use is well developed and in good health. This fact is, in truth, of much significance. It speaks to us of our ancestry. It teaches us under what circumstances man has become what he is. With the food which he used, his system must have been in harmony, and his inclinations, too, are connected therewith. Many years ago I had shown that the harmony of animal life in general finds its explanation in three laws—that of habit, of exercise, and of hereditaryness. The first announces that each animal being is so modified in its organization by the influence to which it is persistently exposed that it harmonises with these influences. The second is, that each organ, each part of the body is so modified under the constant influence of the will or of other circumstances which determine its action, that it corresponds to

what this will or these circumstances require of it. And finally, the law of hereditariness is thus expressed: that the peculiarities of ancestry are transmitted to posterity; this law, therefore, confirms for the subsequent generations what habit and practice have effected in the preceding ones. By these laws, which we find everywhere confirmed by the phenomena,^a all that we call suitable in nature is recognised as necessary; harmony is only a manifestation of these laws. And if we further reflect that defective objects often die prematurely, without propagating their race, and that also in the remaining ones the conditions for propagation are in general less favourable, we find in the laws described at the same time the foundation of progressive perfection in creation. But is retrogression, then, inconceivable? By no means. If the conditions whereby the nature of organic beings is determined, are no longer to be realized, the race is threatened with retrogression, which may terminate in destruction. So man is also, under the influences of over-population and accumulation in great centres, liable to retrogression. Want of the aliments, under the use of which man has become what he is, may be a very essential factor therein.

On another occasion^b I have proposed to myself the question:—What, after a series of generations, would become of man if he gradually accustomed himself to an exclusively vegetable or animal diet? I acknowledged that the results thereof could not be foreseen, but at the same time I showed that it would be a rash struggle of the human will against the course of nature to strive after another harmony, whereby, even if it could be effected, its best qualities and noblest gifts might easily be imperilled. Now from time to time some, impelled by certain philosophical or religious prejudices, have advocated the exclusive use of vegetable food. Perhaps such a doctrine should be opposed with some earnestness if we were not convinced that it will never meet with much countenance. Nature is stronger than theory. To the followers of Pythagoras, who rejected the use of animal food, my friend the late Professor Karsten thought that the “*naturam expelles furcâ, tamen usque recurret*” might also be safely applied. Meanwhile, wonders are related of the sect of the *Vegetarians*. The children of their first apostle, a certain John Newton, who in 1811 made his appearance with his doctrine, were said to excel in health and beauty, and to be perfect models for a sculptor. If the statement is true, is it nevertheless not absurd to connect the beauty of the bodily forms with the diet followed in a single generation? Let natural science beware of extending the hand to anomalies of the religious sentiment.

^a More fully developed in my inaugural address: *De harmonie van het dierlijk leven de openbaring van wetten.* [The harmony of animal life a manifestation of laws.] Utrecht, 1848.

^b *De voedingsbeginselen.* [The principles of nutrition.] 1858, p. 40.

We may now further inquire:—What does daily experience teach respecting the more direct influence of diet? We have thus far spoken of vegetable and animal food. Evidently, however, the question refers only to the proportion of the alimentary principles. In general we may say that fats, and especially albuminous matters, are more fully represented in animal than in vegetable food. But fats are fats, and they will not differ in their action on the animal system, whether they be of vegetable or of animal origin. The same is true of albuminous matters. Therefore, too, if there be any difference, for example, between the use of bread and butter and of meat and potatoes, this will be found rather in the accompanying alimentary principles than in the essential necessary elements—albuminous matters, fats and carbo-hydrates—it being understood that these, in both cases, are present in equal proportion.

Now, experience teaches that a vegetable diet, with excess of carbo-hydrates, promotes the deposition of fat in the body. The animals we fatten are all herbivorous; and for a particular species, too, such as the pig, which belongs to the omnivora, potatoes are quite enough. Even dogs and cats become stout and fat on vegetable food—a proof that it is not so much the kind of animal as the nature of the food which makes the difference; and, just as any other carnivorous animal, they always become, on the most liberal supply of meat (of course without much fat) comparatively lean. In accordance herewith we see excessive deposition of fat in man, too, disappear through the use of animal diet. William Banting,^a like many before and after him, has experienced the truth of this in his own person; and the experiments of Ranke, communicated above (p. 488) supply the key to it. Now, if it be true that in the animal system fatty matters are formed not from carbo-hydrates, but precisely from albuminous matters, as becomes more and more probable, it is sufficiently evident that the formation and deposition of fat are connected with very different conditions.

From our remarks, as well as from the facts adduced, we arrive at the conclusion that all men ought not to be fed in the same manner. Their diet differs in different nations, partly in connexion with the climate they inhabit, partly in connexion with what the country yields, and by mutual interchange both parties would lose. In the same people, too, the feeding must vary according to birth, position, calling, and habit. Even the requirements of the individual must be attended to. With a tendency to excessive deposition of fat, which interferes with

^a This William Banting, a well known and esteemed inhabitant of London (formerly “a furnisher” of the “fashionable world”), in a “Letter on Corpulence, addressed to the Public,” London, 1863, describes with as much simplicity as unmistakable good faith, the favourable influence which animal diet speedily had on the excessive deposition of fat which had for years oppressed him.

many functions, albuminous matters should be freely given, while the use of fats and carbo-hydrates should be limited, and the latter, provided they are well borne, may be advantageous to the lean. An intelligent man will in this respect consult his own experience.

In particular we have finally to ask:—On what diet is man best adapted for muscular work? In connexion with this inquiry I cannot suppress the remark, that in my opinion a too absolute value has been attached to capability of muscular work. It was as if man was to live exclusively for his muscles, and as if, with the acquirement of the greatest muscular power, the most perfect health and the highest development in every direction would also be attained. This partiality has also sought advantages in gymnastic exercises, which I cannot possibly find in them, unless, perhaps, as corrective of the injuries proceeding from constrained sedentariness in schools, against which the natural movements of running, leaping, and wrestling may do quite as much, without giving rise to disproportionate and consequently unpractical muscular development. We cannot overlook that with the increase of civilization the muscular work required of the people gradually diminishes. Everywhere the tendency is to replace this by machine work, for the very valid reason, that mechanical work is under no form so costly as under that of muscular work, especially of men. It is true that we obtain, in reference to the fuel (food) consumed, more useful work than in the best steam-engine;^a but the fuel is amazingly dear in comparison with the price of turf and coal. Thus we should be able to justify the proposition that the worst use to make of a man is to employ him exclusively in mechanical work.

But the question:—On what food man is best suited for mechanical work has in this way by no means lost its importance. Muscular labour is still connected with all work, and in some kinds of work powerful exertion of the muscles is always involved—digging, ploughing, and thrashing, the hoisting or carrying up of heavy loads, the handling of heavy hammers or hatchets, rowing, sawing, ramming, &c.; also long marches, with bag and baggage, and various exercises in the management of arms, are to be enumerated. Moreover, muscular work, in a certain measure, will always be required in the interest of the organism. That albuminous matters are in the first place necessary for this purpose, and that they should be more liberally represented in the food of the working man than is the case with us, was some years ago argued by Mulder with an enthusiasm which was the result of a deeply-felt conviction. This conviction communicated itself to all. On no side was it opposed. It was not asked whether muscular work was produced directly from the albuminous matters—the law of the maintenance of energy had as

^a Conf. Helmholtz, Lectures. *L. c.* Lecture vi.

yet found but little application: but he reasoned simply thus: the muscles must perform work, and they must therefore, in the first instance, be well nourished, and indeed with albuminous matters, of which they are chiefly composed. Now, after it was on the one hand proved that non-nitrogenous matters were also capable of producing mechanical work, and after it was on the other hand shown that in this work a proportionate metamorphosis of albuminous matters does not always take place, some began to abandon their conviction. And soon they now came forward with the assertion that heavy work is performed with a slight supply of albumen, and that it is precisely the consumers of albumen who in general use their muscles but little. The last statement is perfectly true; it proves, however, as we have already said, only that also without muscular work much albumen may be metamorphosed. The first we deny. Science has led us to infer that a supply of albuminous matters makes the muscles better adapted for work, and daily experience fully confirms this. The argument made use of we reject for the following reasons:—

1. People are deceived as to the amount of work performed. Most callings require rather an accurate use than great tension of the muscles. In fact the work performed by the human hand is, when measured by the accurate standard of kilogrammeters, in general very little. If the manufacturer of fabrics was called to perform hard work, to carry heavy loads, or to overcome great resistance, it would soon be seen how little he is fitted for the purpose. Even his very appearance betrays it. On slender food and under unfavourable circumstances his physical condition is lowered to a degree at which it can now be maintained by that slender diet. He gets little, especially of albuminous matter, and has scarcely need of more (compare p. 487). At the same time he has lost the bodily form, the shape, the weight even of the normal man. For him special statistics exist. Thus he has the true capability for working in his manufacture. Involuntarily he appears before our mind's eye when we read in Bruecke:—"Scantily nourished on thistles and weeds, the ass has become the slave of the poor man, who feeds his beast by the wayside: he is the *proletary* among the beasts of burden, in whose degraded form we no longer recognise the strong and beautiful animal who formerly, decorated with costly trappings, bore on his back the princes of the East;" and the "*quantum mutatus ab illo*" is on our lips as we think of the old Batavians.

- 2°. They who actually perform work also use albumen, and in general consume more in proportion to the severity of the labour *a.* The quantity of food is great, surprisingly great, we will say, if we visit the tables of the country people, who do heavy work. Proportionate thereto is the albumen ingested. *b.* They get more albuminous matters than one suspects. Besides potatoes they use different kinds of flour and

bread in great quantity, and every evening trencherfuls of buttermilk, which contains all the casein of the milk; and in the middle of the day pork and bacon. For soldiers in garrison Mulder calculated 100 grammes of albuminous matters daily; sailors receive still more. c. In a yard visited by me some men were employed the greater part of the day sawing planks. The strongest were chosen for it. They got higher wages and required it, for "without a pound of meat daily and a good jug of beer one could never hold out at that work." Thus, too, the guide, who daily ascends mountains, helps the traveller and carries his valise, must be well fed and especially requires animal food. Of the English labourer, who daily gets meat and drinks strong beer, we know that he is strong and active, and the Irish people, who live almost exclusively on potatoes, are called lazy and slothful. I have often remarked that slothfulness is rather a morbid symptom than a vice.

The conclusion is this:—Muscular work and heat arise in the animal organism, both being derived from the chemical energy as well of non-nitrogenous as of nitrogenous matters. Of both kinds of food the animal system has need. In the body there exists a certain relation between heat produced and muscular work. By exercise this relation becomes more favourable for muscular work. A liberal supply of albuminous matters tells favourably in the same. The reason of this is probably to be found in the better nourished and firmer condition of the muscles and of the whole body, which is obtained by means of a more highly albuminous diet. The development of man in general appears to attain the highest pitch under the use of a mixed diet.

CLINICAL RECORDS.

LONDONDERRY CITY AND COUNTY INFIRMARY, AND CITY FEVER HOSPITAL.

REPORT FOR 1865.

By T. H. BABINGTON, M.D., T.C.D., M.R.I.A., Surgeon to the Infirmary.

Return of Patients admitted into and Treated in the City and County of Londonderry Infirmary, during the year ending 31st December, 1865.

Remaining 31st December, 1864,	53
Admitted,	571—Total, 624
Discharged, cured and relieved,	511
Irregular,	2
Left at own desire,	31
Incurable,	6
Died,	35—Total, 585
Remaining 31st December, 1865,	39

City of Londonderry Fever Hospital.	
Remaining 31st December, 1864,	26
Admitted,	268—Total, 294
Discharged cured,	257
Died,	20—Total, 277
Remaining 31st December, 1865,	17
Number of Beds in Hospital,	48 Male.
Ditto,	24 Female.
Total,	72
Number of Beds in Fever Hospital,	32
Total number of Days passed by Patients in Hospital,	14,777
Total number of Days passed by Patients in Fever Hospital,	7,057
Average Cost of Hospital and Fever Patients, including all expenses,	£1 15 5½
Average Cost of Hospital and Fever Patients, exclusive of Salaries, and Wages, amounting to £506 2s. 9d.,	1 4 5
Daily Cost of each Patient, including all expenses,	0 1 6
Daily Cost of each Patient, exclusive of Salaries and Wages, amounting to £506 2s. 9d.,	0 1 0½

Numerical Abstract of Cases of Accidents and Diseases admitted in year 1865.			
ACCIDENTS.	No.	ACCIDENTS.	No.
Burns and Scalds,	6	Fracture of Leg,	6
Bite of Dog,	2	" Ribs,	2
Dislocation of Hip,	1	" Thigh,	6
" Shoulder,	3	Compound Fracture of Arm,	1
" Tibia,	1	" Leg,	1
Fracture of Acromion,	1	Injuries of Head,	19
" Forearm,	8	Rupture of Stomach,	1
" Humerus,	3	Ununited Fracture of Humerus,	1
" Clavicle,	1	Wounds and Contusions,	86
" Lower Jaw,	1	Wound of Throat, suicidal,	1
DISEASES.	Cases.	DISEASES.	Cases.
Abscess,	10	Diseases of Testicle,	4
Ague,	2	" Uterus,	11
Anemia,	3	Erysipelas,	7
Aneurism of Aorta,	1	Excessive Intoxication,	2
" Popliteal Artery,	1	Fistula in Ano,	1
Anthrax,	3	Hernia,	6
Cancer of Breast,	1	Hare-lip,	2
" Face,	3	Hydrocele,	4
" Lip,	7	Ovarian Disease,	1
" Stomach,	2	Paronychia,	10
Diabetes,	2	Polypus of Nose,	1
Diseases of Brain, &c.,	21	Retention of Urine,	8
" Bones and Joints,	18	Rheumatism,	25
" Eyes,	23	Struma,	6
" Heart,	4	Syphilis,	19
" Lungs, &c.,	67 ^a	Tumours,	6
" Stomach, Liver, &c.,	53	Ulcers,	33
" Skin,	34	Fever,	268

^a Including bronchitis, 34 ; phthisis, 25 ; pleuritis, 5 ; pneumonia, 3.

Diseases and Accidents which proved Fatal.

	Died.		Died.
Diseases of Brain,	2	Heart Disease,	1
Albuminuria,	3	Injuries of Head,	3
Aneurism of Aorta,	1	Phthisis,	6
Bronchitis,	4	Pneumonia,	2
Dropsy,	2	Periostitis, &c.,	1
Dysentery,	1	Ovarian Disease,	1
General Debility,	1	Rupture of Stomach,	1
Hernia,	1	Urinary Fever,	1
Hepatitis,	1	Tabes Mesenterica,	1
Hectic Fever,	2	Fever,	20

Operations.

	No.
Amputations of Thigh,	2, one died.
Amputation of Breast,	1, for cancer.
Amputation of Thumb,	1, for exostosis.
Excision of Half of Lower Jaw,	1.
Operation for Strangulated Hernia,	1, died four hours after.
Operation for Contraction of Fingers from burn,	1, cured.
Removal of Cancerous Lips,	7.
Operations for Hare-lip,	2.

Some of the cases in foregoing abstract are deserving of a more extended notice. It will not be inconvenient to remark on each in its numerical order:—

1. *Dislocation of the Tibia.*—This accident occurred to an elderly man falling over the curb-stone of the flagging, as he came out of a hall-door at night. The fibula was fractured, and the tibia dislocated forwards on the astragalus. He came under treatment a fortnight after the accident. The dislocation was reduced with some difficulty, the limb firmly bandaged with a splint behind the heel, and all placed on a M'Intire's splint. The patient made a good recovery, and is walking about with but a slight halt.

2. *Injuries of the Head,* at all times formidable, and even the most trifling, demand serious attention and utmost vigilance. We had three fatal cases during the year:—The first, a passenger on board an emigrant ship, sailed for New York on 24th May, 1865. The ship came to anchor about sixteen miles down the river, and about ten o'clock, p.m., the patient was found lying in the hold, where the passengers' luggage had been stowed, totally insensible, having fallen down the hatchway. He was brought back to Londonderry in a steamer, and received into hospital at four o'clock, a.m., on the 25th. He was totally insensible, very cold, and almost pulseless—in fact, I thought he was dying. Heat was applied to his extremities and the whole surface of his body, and some warm wine and water administered in such quantities as could be got down his throat. In a few hours he was somewhat restored, his

pulse improved, but he still remained speechless and insensible. The frontal bone, and a portion of the parietal on right side, were fractured and depressed. A crucial incision was made over these, the flaps dissected up, the fractured and depressed pieces of bone removed; there was no blood extravasated on the dura mater. The patient immediately spoke, told his name, where he lived, and the name of his clergyman. He continued sensible, and we had good hopes of his recovery; but on the third day he became stupid and insensible, passed his urine and feces under him, and died on the evening of the 28th May. On examination there was a considerable quantity of pus under the dura mater, and the fractures had extended down through the temporal bone and all along the parietal, but all the depressed portions had been removed.

The second fatal case was the result of concussion. A horse ran away with a cart, which was overturned, and the patient tossed out, his head striking against a wall. He never rallied, remained totally insensible, and died in thirty-six hours. There was no fracture, nor any extravasation of blood.

The third case (fracture).—A sailor fell from the mast of his vessel, and sustained a compound fracture of right humerus, and severe injury of head. On admission four hours after the accident he was totally insensible, bleeding from right ear—and, in fact, dying. He expired three hours after his admission. The frontal, temporal, and parietal bones on right side were all fractured, and blood extravasated under the cranium, on the surface of the dura mater.

Rupture of Stomach.—A stout healthy young man, attending masons working at new gas works in course of erection, fell from a scaffold near the roof, a height of about forty-five feet. How he fell no one could tell; it was supposed his head struck against some piece of timber in his fall, as there was a severe contusion on his right temple; he was found on the ground in a doubled-up posture, his right side lying against some blocks of stone. He was brought to hospital at once, about one o'clock, p.m. He complained of intense agony all over his bowels; he was vomiting, nearly pulseless, his skin quite pale, and icy cold; no pain in his head, nor any confusion of intellect—in fact, his symptoms were intense collapse, and violent pain all over the stomach and abdomen, and he exclaimed:—"There is something burning there," pointing to his stomach—"take it out of that." He expired about two hours after his admission. On examination there was found a rent in his stomach, on the anterior surface, about two inches long, a complete tear, situated about three inches from the pylorus, and through this opening the contents of the stomach, which looked like stirabout, had escaped. The stomach was perfectly sound in every other respect.

Ununited Fracture of Humerus, cured by seton.—A stout muscular man, a carter, admitted into infirmary on 26th December, 1865. He had got

his humerus fractured four months before; he said he could make no use of it. On examination it was evident that union had not taken place; the ends of the bones were not in apposition, they lay sideways by each other, the space between them filled with a substance feeling like cartilage, but soft, and he was unable to lift his arm. On the 1st January, with the assistance and concurrence of my friends, Dr. White and Dr. Rogan, I passed a seton (a skein of silk) through the arm, between the ends of the bones. An interval of ten days elapsed before much inflammatory action set in, subsequently an abscess formed, and a second one, with a good deal of irritation, and at the end of the third week I withdrew the seton. The arm was kept firmly adjusted in splints, the situation of the seton being kept uncovered. I have had the satisfaction of sending him home to a distant part of the county, on the 20th March, with a firmly united arm. During the treatment he was generously fed, and took fifteen grains super-phosphate of lime, three times a day.

Aneurism of Aorta.—The patient in this case had been long under observation. He became blind before his death, and finally sunk under a long protracted attack of chronic pleuritis.

Aneurism of Popliteal Artery, Cured by Compression.—I had the gratification of publishing, in Vol. XXIV. of *Dublin Quarterly Journal of Medical Science*, "Notes of a Case of Popliteal Aneurism, Cured by Compression;" the treatment of the case extended from 20th August till 29th September. I have now the pleasure of recording another successful case of same disease, cured by compression, but in which the treatment, although in the end successful, was protracted to an unusual extent. I was, however, in the result amply rewarded for patience and perseverance. It would be tedious and unprofitable to detail minutely the daily notes of a case, the treatment of which extended from the 28th September, 1865, when the pressure was first applied, till the 30th January, 1866, the day on which the disease was cured:—William Neely, a sawyer by trade, a pale anemic-looking man, aged thirty-five, had always enjoyed fair health, but had the constitutional defect that the slightest scratch or wound was always peculiarly slow in healing; his blood seemed weak and poor, and deficient in fibrin and red globules. While engaged with others in lifting a log of timber, the log rolled back on them; he made additional exertion to resist it, and felt an uneasiness not amounting to a pain in his left leg, behind the knee. He went to his work the next and following day; and the third day, his limb feeling very stiff, he consulted my friend, Dr. Edward Smith, who detected the existence of an aneurism in the left popliteal space, and recommended him to come into hospital, which he did. He was placed on a hair mattress, ordered good diet, porter, and not much fluid, and an opium pill every sixth hour. On the 28th September, at noon, the circular compressor was placed round his thigh, and commanded the circulation completely; he wore it screwed down for

two and a half hours. It was kept on for four days and nights, but the pressure had to be slackened every two or three hours; it was then removed, the skin having ulcerated. Cartes' saddle was put on, and pressure applied below Pourpart's ligament. He with difficulty bore this pressure for two hours at a time; it had constantly to be slackened, and on the 16th October it had to be removed, the skin in the groin having ulcerated, and the whole limb having become œdematous. Notwithstanding the frequent interruptions to the treatment, the tumour had acquired a feeling of considerable firmness and solidity. He was now left quiet, without any treatment, except firmly flexing the leg and keeping it so; this produced no good result. He complained so much of the uneasiness caused by Carte's compressor, that I procured one of Read's compressors, which I applied on 4th November. He bore this better, but could not bear continued pressure for more than three or four hours; the pressure was kept up at intervals till 21st December, when he left hospital, and returned 5th January. On the 6th January, Read's compressor was again applied, and worn with little interruption for three days and nights. He was also ordered fifteen grains saccharated carbonate of iron three times a day. On the 10th we had to remove the instrument, but there was a marked improvement in the tumour; it was becoming solid, circumscribed, and the pulsation very feeble. The abraded surface having healed, the compressor was again applied, on the 26th January, and screwed down. On the morning of the 30th, on visiting the man, he said:—"Please, sir, examine my leg, for at four o'clock this morning I felt a most unusual sensation in it, as if I was stabbed through the joint; and I called to the patient opposite; there is something very queer in my leg." On minutely examining his leg, all pulsation had ceased. He remained in hospital for ten days afterwards, and is now daily at his work.

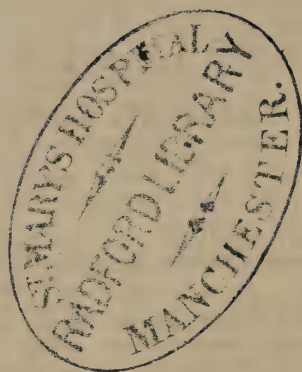
In the treatment of this case I was frequently disheartened and discouraged, often hopeless as to the result, and more than once disposed to tie the artery, but deterred by the weak constitution I had to deal with. We have, however, been rewarded for our patience and perseverance. I beg to bear testimony to the efficacy of Read's instrument.

Ovarian Disease ; fatal.—The notes of this case, and the morbid specimens, were brought before the Obstetrical Society by my friend, Dr. Denham, at the meeting of 10th March.

Periostitis and Synovitis of Left Humerus.—A case of this formidable disease—several cases of which are graphically described by the master-hand of the late Mr. M'Dowel in the numbers of the *Dublin Journal of Medical Science* for the months of July and September, 1833:—Thomas M'Laughlin, aged eighteen, admitted into hospital 26th July; complained of pain in his left shoulder; said he had sprained it two weeks since, wheeling a barrow; had rubbed it with turpentine, but it was no better.

On examining it, he complained of excruciating pain, greatly increased by pressure and motion; the parts round the shoulder joint were swollen and tense, and the skin rather red. He, on being questioned, said he had suffered very much pain, and got no sleep; he was very thirsty, and had all the general symptoms of high inflammatory fever, and much constitutional disturbance. I at once said to Dr. Rawson, my house-surgeon, now surgeon to Baltinglass Infirmary and Dispensary—"That is no case of sprain, but a case of that formidable affection of the periosteum and synovial membrane described by late Mr. M'Dowel, and I am afraid will end fatally." A large number of leeches were applied over the joint, and around it; warm fomentations; calomel and opium administered internally. Next day there was no improvement; the leeches were again applied. On the fourth morning, the 29th, there being a feeling, but an indistinct one, of fluctuation, I made two deep incisions over the joint, and along the deltoid muscle; little, very little pus escaped, but the tension was relieved. The following morning, the 30th, I made another incision, and on passing a director into the incision it went direct into the joint; the synovial membrane had given way, and the periosteum was detached from the head of the bone. 30th. He complained this morning, the sixth from his admission, of acute pain in his right side, and his breathing was very rapid; inflammation had set in in the lower lobe of his right lung. He sank rapidly, and died on the morning of 1st of August.

The examination after death revealed appearances as described by Mr. M'Dowel:—The muscles infiltrated with pus, synovial capsule of joint opened, periosteum detached from head, neck, and shaft of humerus, and the head of the bone corroded; the lower lobe of right lung inflamed and filled with numerous, small, distinct, purulent deposits.



INDEX

TO THE FORTY-FIRST VOLUME.

- ACONITE** in puerperal fever, 218.
Adams W., on curvature of the spine, &c., *Rev.*, 160.
Addison's disease, Hayden on, 195.
Age, advanced, diseases and infirmities of, 134—blood-letting in, 137—changes in respiratory organs in, 146—chronic enlargement of prostate, 153—climacteric disease, 142—condition of heart in, 150—meningitis in, 144—therapeutics of, 137.
Air, rebreathed, as a cause of consumption, 414.
Albuminuria, 467.
Andes, climate of Swiss and Peruvian Andes compared, 339.
Amenorrhea from imperfect development of the uterus, 222—use of galvanic pessary in.
Amputation at hip-joint, 365.
Anchylrosis of knee, forcible flexion, 10.
Andes, climate of and Swiss Alps compared, 339.
Aneurism of left common iliac artery and ligature, 192—popliteal cured by compression, 561—varicose aneurism, 434.
Aorta, rupture of, 202—varicose aneurism, 434.
Apoplexy, sun-stroke or heat apoplexy, 122.
Arachnitis, cerebro-spinal, 448.
Arnold, elements of physics, *Rev.*, 155.
Artery, ligature of left common iliac, 192—rupture of aorta, 202.
Ascaris, lumbricoides, in trachea, 284.
Asthma, Dobell on, *Rev.*, 409.
Ataxy, progressive motor, 304.
Atmosphere, conditions of, influencing prevalence of typhus fever, 309.
Atthill, inflammation of bladder and vagina, 456.
Aural surgery of the present day, *Kramer on*, *Rev.*, 369.
Australia, climate of in consumption, 409.
Babbington, disease of ovary, 455—report of Londonderry Infirmary and Fever Hospital, 497.
Banks on cerebro-spinal arachnitis, 448.
Baxter on sun-stroke or heat apoplexy, with cases, 122.
Beef, South American, 188.
Belcher, Neligan on diseases of skin, edited by, *Rev.*, 154—remarks on diphtheria, 314.
Bennett, doctrines of on pneumonia criticised, 462.
Bigg on orthopraxy, the mechanical treatment of deformities, &c., *Rev.*, 194.
Bladder, on crushing stone in, 1, 99—and vagina, inflammation of, 456.
Bleeding, in diseases of advanced life, 139.
Bread, as an article of diet, 187.
Brodhurst, on curvatures of the spine, *Rev.*, 160.
Bromidrosis, or fetid perspiration, 327.
Bronchitis, Dobell on, *Rev.*, 409.
Brown, I. B., Jun., on Australia for consumption, *Rev.* 409.
Butter, of cocoa-nut, 222.
Carpenter, human physiology, by Power, *Rev.*, 155—manual of physiology, *Rev.*, 155—on microscope, *Rev.*, 155.
Catarrh, Dobell on, *Rev.*, 409—suffocative, treatment of in old age, 139.
Cerebro-spinal arachnitis, 448.
Chambers, lectures chiefly clinical, *Rev.*, 154.
Chloroform, effect of temperature on diffusion of, 355—Sansom on its action and administration, *Rev.*, 354.
Chromidrosis, 64.
Churchill, address to Dublin Obstetrical Society, 208—on improvements in midwifery during last 30 years, 209.
Cirrhosis, of liver, 443—of lung, 205, 206.

- Clavicle, compound fracture of, 10.
 Climate, of Australia for consumption, 409—of Swiss Alps, and Peruvian Andes compared, 339.
 Climateric decay, 142.
 Clinical records, 497—report of Derry Infirmary and Fever Hospital, 497.
 Cocoa-nut, butter of, 222.
 Colles, cases of injuries of larger joints, with remarks, 49.
 Collis, fibrous tumour of arm, 201.
 Conjunctivitis, granular, 38.
 Consumption, Australia for, *Rev.*, 409—climate of Peruvian Andes for, 349—Drysdale on antecedents and treatment of, *Rev.*, 409—M'Cormac on, as engendered by rebreathed air, *Rev.*, 409—Pollock on elements of prognosis in, *Rev.*, 409—poverty as a cause of, 410.
 Copland, dictionary of medicine, new edition, *Rev.*, 154.
 Cork Medical and Surgical Society, transactions of, 228, 462.
 Croly, malignant tumour of the neck, 431.
 Cyphosis, 163.
 Day, notes on delirium tremens, 228.
 Deaf and dumb, laryngoscopical observations on, 425.
 Deformities, Adams and Brodhurst on, *Rev.*, 160—mechanical treatment of, 194.
 Delirium tremens, notes of by B. W. Day, 228.
 Denham, case of labour with unruptured hymen, 228.
 Dictionary, Copland's of Med., *Rev.*, 154—Dunglison's, 155.
 Dietary, Smith's practical for families, &c., *Rev.*, 186—relation of food to work, &c., 238, 469.
 Diphtheria, Belcher on, 314—Fleming on, 441.
 Dislocation of tibia, 499.
 Dobell, on winter cough, *Rev.*, 409.
 Donders, on constituents of food, and their relation to muscular work, and animal heat, 238, 469.
 Dumb, laryngoscopical observations on, 425.
 Dunglison, medical dictionary, *Rev.*, 155.
 Drysdale, on antecedents and treatment of consumption, *Rev.*, 409.
 Duffey, on rupture of aorta, 202.—cystic diseases of kidneys, 438.
 Duodenum, ulceration of, 90.
 Emphysema, Dobell on, *Rev.*, 409.
 Empyema, drainage in, 83.
 Enchondroma of hand, 199.
 Endoscope, examination of pleura with, 83.
 Entozoa in kidney, 391.
 Epidemics, Howe on physical causes of, *Rev.*, 395.
 Epulis, Fleming on, 429.
 Eye, treatment of granular ophthalmia, 38—W. Jones on diseases of, *Rev.*, 155.
 Farre, abridgment of Pereira's materia medica, *Rev.*, 362.
 Fever, atmospheric conditions influencing prevalence of typhus, 309—treatment of puerperal, 218.
 Fingers, clubbed in pleuritis, 90—in phthisis, 421.
 Finn, Albuminuria, gall stone, and suppression of urine, 467.
 Fistula, reno-pulmonary, 90.
 Fleming, on diphtheria, 441—on epulis, 429—cases of lithotripsy, and lithotomy, 99.
 Food, constituents of, and their relation to muscular work and animal heat, 238—of man, 492—of working animals, 489.
 Foot, on chromidrosis, 64—on cirrhosis of lung, 206—on bromidrosis or fetid perspiration, 327—on gynecomazia, 457.
 Fracture, comp. of clavicle, 10—united, cured by seton, 500.
 Gamgee, on amputation at hip joint, *Rev.*, 365.
 Gastrotomy, after rupture of uterus, 278.
 Glottis, case of contraction of, 335.
 Gordon, clin. reports of rare cases occurring in the Whitworth and Hardwicke hospitals, 83—hypertrophy of the heart from renal disease, regurgitation independent of valvular disease, 95—pleuritic effusion, drainage, examination of pleura by the endoscope, 83—reno-pulmonary fistula.
 Grimshaw, on atmospheric conditions influencing the prevalence of typhus fever, 309.
 Gross, system of surgery by, *Rev.*, 155.
 Gynecomazia, Foot on, 451.
 Hargrave, on ligature of the left common iliac artery, *Rev.*, 192.
 Hayden, on disease of supra-renal capsules, 195—hypertrophy of heart and cirrhosis of liver, 443—on typhoid pneumonia associated with tympanitic dulness, 22—varicose aneurism, 434.

- Head, injury of, 499.
- Health, philosophy of, by S. Smith, *Rev.*, 156.
- Heart, condition of, in advanced age, 150—disease of mitral, 432—do., combined with aortic valve disease, 203—hypertrophy and cirrhosis of liver, 443—hypertrophy from renal disease, 95, 44, 468—malformation of valves, 392—Peacock, on causes and effects of valvular diseases of, *Rev.*, 392—regurgitation independent of valvular disease, 95.
- Heat, relation of food to animal, 238.
- Hernia, Salt on, *Rev.*, 194—strangulated femoral with irreducible omentum, operation, 10.
- Howe, physical cause of epidemic diseases, *Rev.*, 395.
- Hygiene, Parkes' manual of practical, *Rev.*, 397.
- Hymen, labour with unruptured, 228.
- Jennings, on cirrhosis of lung, 205.
- Joint, amputation at hip, 365—ankylosis of knee, forcible flexion, 10—cases of injuries of large, 47—Price, on excision or amputation of knee, *Rev.*, 398—shoulder, resection of, 365.
- Jones, W., on ophthalmic med. and surgery, *Rev.*, 155.
- Kidd, amenorrhea from imperfect development of the uterus, 222—galvanic pessaries, use of, 222—medicated pessaries, 226.
- Kidney, calculus and abscess in, 90—cystic disease of, 438—disease of supraprenal capsule, 195—hypertrophy of heart in renal disease, 95—reno-pulmonary fistula, 90—Roberts on diseases of, *Rev.*, 386.
- Knee, ankylosis, forcible flexion, 10—Price on excision and amputation of, *Rev.*, 378.
- Kramer, aural surgery of the present day, *Rev.*, 369—*Ohrenkrankheiten und Ohrenärzte in England und Deutschland*, *Rev.*, 369.
- Kreatine, amount of, in muscles, 402.
- Labour, mechanism of, 182—with unruptured hymen, 228—laceration of vagina in, 257—obstructed by cystic enlargement of kidney, 438.
- Laryngoscope, cases illustrating usefulness of, 335—observations on deaf and dumb with, 425.
- Larynx, changes of in advanced age, 146—contraction of rima, 335—mode of fixing while operating within, 10—removal of arytenoid, 336—tubing of, 285.
- Law, on cerebro spinal meningitis, 288—on disease of mitral valve, 203, 432.
- Lectures, clinical, by Chambers, *Rev.*, 154.
- Leishman, on mechanism of parturition, *Rev.*, 172.
- Life, diseases and infirmities of advanced, 134—duration of human, 136.
- Ligature of left com. iliac artery, 192.
- Lithotomy and lithotrixy, cases of, by Fleming, 99.
- Lithotrixy, cases of, by Fleming, 99—Porter on, 1.
- Liver, cirrhosis of, and hypertrophy of heart, 443.
- Longmore, on resection of shoulder, *Rev.*, 365.
- Lordosis, 161.
- Lung, cirrhosis of, 205, 206—condition of in advanced age, 146—inflammation of in advanced age, 148—recent works on diseases of, *Rev.*, 409—reno-pulmonary fistula, 90.
- Lyons, gangrene of œsophagus, 445.
- McClintock, on laceration of vagina in labour, 257—on scarlatina complicating childbed, 53.
- McCormac, on consumption as engendered by re-breathed air, *Rev.*, 409.
- McDonnell, cases illustrative of the usefulness of the laryngoscope, 335.
- McLaughlin, a practical treatise of disease and infirmities of advanced life, *Rev.*, 134.
- Malta, hygienic condition of, 399.
- Mamma, enlargement of in males, 451.
- Materia medica, Pereira's treatise on, abridged by Farre, *Rev.*, 362.
- Meningitis in advanced age, 144—cerebro-spinal, 288.
- Microscope, Carpenter on, *Rev.*, 155.
- Midwifery, definition of terms used in, 177—progress of during last 30 years, 209.
- Milk, secretion of coloured, 71.
- Moore, W., on syphilitic paralysis and its treatment, 300—W. D., translation by of Donders on food, 238, 469.
- Morgan, meat-preserving process, 188.
- Muscle, chemistry of, 401, 469.
- Neligan, on diseases of skin, by Belcher, *Rev.*, 154.

- Obstétrical Society of Dublin, proceedings of, 208, 451.
- O'Connor, on treatment of pneumonia, 462.
- Œsophagous, foreign body in, and gangrene of, 445.
- Ohrenkrankheiten und Ohrenärzte, Kramer on, *Rev.*, 369.
- Ophthalmia, granular, 38.
- Orthopraxy, mechanical treatment of deformities, &c., *Rev.*, 194.
- Ovariectomy, Smyly on, 10.
- Ovary, tumour of, 199, 453, 455.
- Paralysis, syphilitic, 300.
- Paraplegia, from meningitis of spinal cord, 229.
- Parkes, manual of practical hygiene, *Rev.*, 397.
- Pathological Society, proceedings of, 195, 429.
- Parturition, mechanism of, 172.
- Peacock, on causes and effects of valvular diseases of the heart, *Rev.*, 392.
- Peet, on principles and practice of medicine, *Rev.*, 367.
- Pericarditis, 432.
- Periostitis and synovitis, 503.
- Perspiration, coloured, 64—fetid, 327.
- Pessaries, galvanic, 222—medicated, 226.
- Phthisis (see Consumption).
- Physicians, register of Col. of in Ireland, *Rev.*, 368.
- Physics, elements of, by Arnold, *Rev.*, 155.
- Physiology, Carpenter's, *Rev.*, 155.
- Pleura, examination of, with endoscope, 83—drainage tube used in, 83.
- Pleuritis, clubbed fingers in chronic, 90.
- Pneumonia, treatment of, 462—Dr. Bennett's views criticised, 462—in advanced age, 148—typhoid, with tympanitic dulness, 22.
- Pollock, elements of prognosis in consumption, *Rev.*, 409.
- Porter, Mr., on enchondroma, 199—observations on crushing stones in the bladder, 1.
- Presentation, definition of the term in midwifery, 177.
- Price on conditions which require amputation or excision, *Rev.*, 378.
- Prostate, state of, in advanced age, 152.
- Pus, coloured, 70.
- Ranke, on tetanus, *Rev.*, 401.
- Roberts, on urinary and renal diseases, including urinary deposits, *Rev.*, 386.
- Rupture, practical treatise on, by Salt, *Rev.*, 194.
- Salomonsen, laryngoscopical observations on deaf and dumb, *Rev.*, 425.
- Salt, practical treatise on rupture, &c. 194.
- Sansom, on chloroform, its action and administration, *Rev.*, 354.
- Scapula, excision of, 365.
- Scarlatina, complicating child-bed, 53.
- Scoleosis, 163.
- Shoulder, resection of, 365.
- Skin, coloured perspiration, 64—hemorrhage from, 66—Neligan, on diseases of, by Belcher, *Rev.*, 154.
- Smith, climate of Swiss Alps and Peruvian Andes, compared, 339.—Ed., practical dietary for families, schools, and the labouring classes, *Rev.*, 186—S., philosophy of health, &c., 155.
- Smyly, *ascaris lumbricoides* in trachea, 284—comp. fracture of clavicle, 10—forcible flexion for ankylosis of knee, 10—mode of fixing larynx while operating within its cavity, 10—notes in medicine and surgery, 10, 284—operations for strangulated femoral hernia, 10—on ovariectomy, 10.
- Spinal cord, inflammation of membranes of, 229.
- Spine, curvatures of, Adams and Brodhurst on, *Rev.*, 160—normal curves, 161—mechanical treatment of deformities of, 194.
- Stokes, Mr. W., on granular ophthalmia, 38.
- Stomach, rupture of, 500.
- Stone in bladder, crushing of, 1, 99.
- Sugar, use of in gangrene, 49.
- Sunstroke, 122.
- Surgery, system of, by Gross, *Rev.*, 155.
- Sutro, on German mineral waters, *Rev.*, 155.
- Syme, on excision of scapula, *Rev.*
- Syphilis, paralysis from, 300.
- Tea, as an article of diet, 189.
- Telford, on treatment of puerperal fever, 218—case of ovarian tumour, paracentesis, death, 457.
- Tellurium, effects of, 328.
- Tetanus, Ranke on, *Rev.*, 401.
- Tibia, dislocation of, 499.
- Townsend, E. R., Jun., on paraplegia, 229.
- Trachea, *ascaris lumbricoides* in, 284.
- Tufnell, on ovarian tumour, 199—scirrhus tumour in mammary region, 429.
- Tumour, fibrous of arm, 201—malignant of neck, 431—ovarian, 199—scirrhus in mammary region, 429.

Urine, coloured, 72, 77—Roberts on urinary and renal diseases, *Rev.*, 386.
Uterus, imperfect development of, 222—rupture of, 227.

Vagina, laceration of, in labour, 257—and bladder inflammation of, 456.

Venesection, in diseases of advanced life, 137.

Vertex, definition of term in midwifery, 177.

Waters, Sutro on German mineral, *Rev.*, 155.

Weather as influencing spread of typhus fever, 309—in connexion with diphtheria and cattle plague, 314.

Winter cough, Dobell on, *Rev.*, 409.

Work, relation of food to 238, 469.

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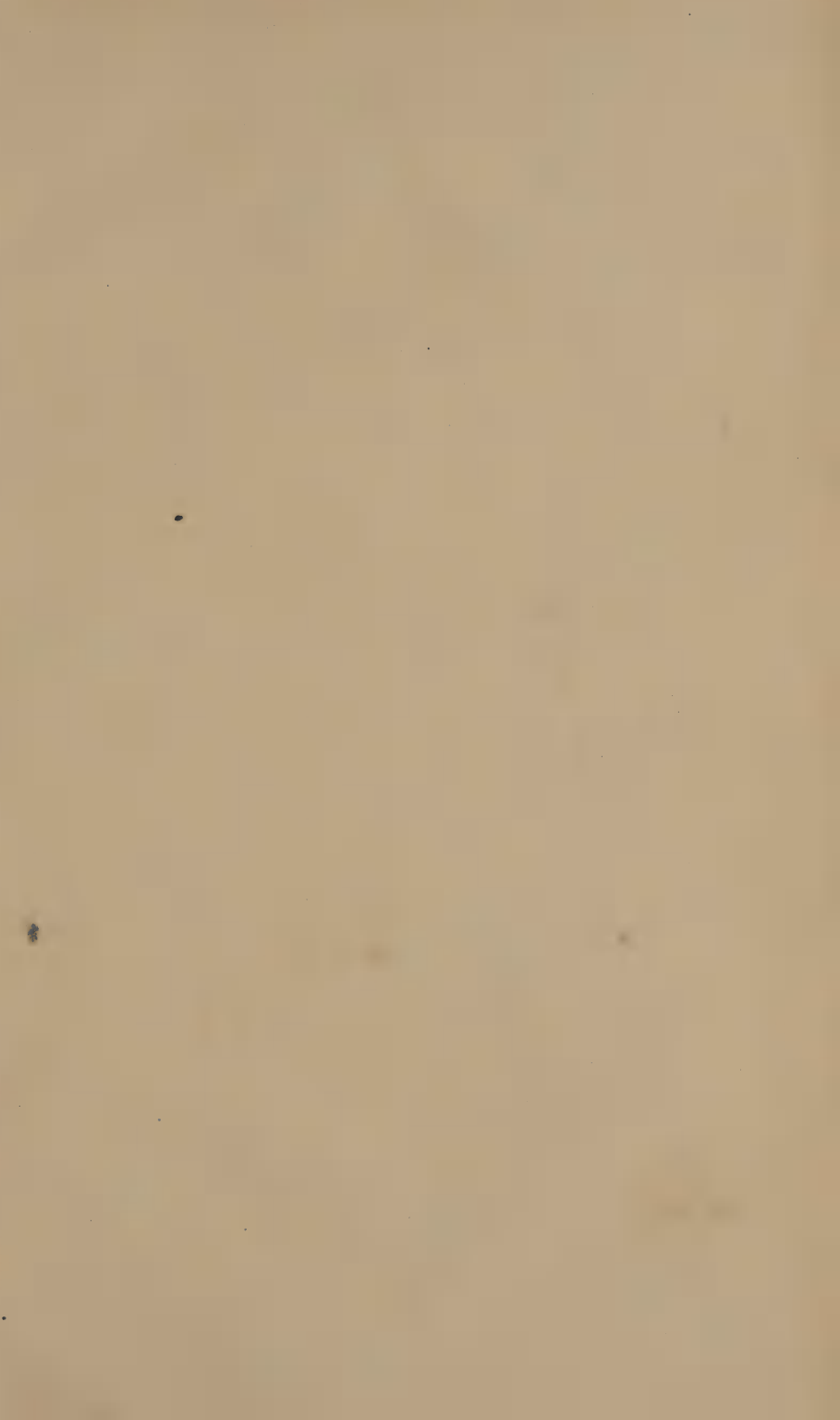
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